Electromagnetic Theory

A Critical Examination of Fundamentals

(formerly titled: Electromagnetics)

by Alfred O'Rahilly

in two volumes VOLUME TWO

O'Rahilly

Electromagnetic Theory

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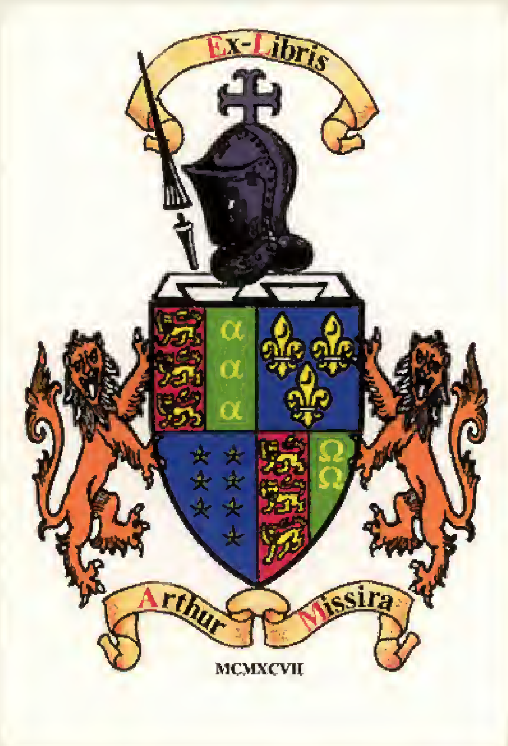
ELECTROMAGNETIC THEORY



ELECTROMAGNETIC THEORY

A CRITICAL EXAMINATION OF FUNDAMENTALS

(formerly titled: Electromagnetics)

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in two volumes

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CONTENTS TO VOLUME TWO

CHAPTER X

Dielectric and Magnetic Bodies

CHAPTER XI

Weber—Ritz

CHAPTER XII

Moving Charges

CHAPTER XIII

The Aether

CHAPTER XIV

The Symbols of Physics

CHAPTER XV

Units and 'Dimensions'

ELECTROMAGNETIC THEORY

CHAPTER X

DIELECTRIC AND MAGNETIC BODIES

1. The Scalar Potential.

While $\varkappa$ and μ are important and even fundamental constants in Maxwell's theory, they become in the electron theory secondary formulae resulting from the statistical effects of electric polarisation and Amperian molecular currents. The only effect of matter is that, in addition to the external charges, we have to deal with unknown internal charges.

The electron theory introduces discontinuities into what Maxwell regarded as a continuum. To avoid the analytical difficulties involved and to justify the use of continuous functions, we adopt an expedient which we now proceed to explain in a simple way which will satisfy the physicist, though of course the pure mathematician will have further scruples. We shall divide the scale of spatial and temporal quantities into three orders of magnitude.

(1) At one extreme we have the *macro-domain*, containing quantities accessible to our senses and to laboratory measurements.

(2) At the other extreme we have the *micro-domain*, i.e. linear dimensions of the order of the interdistances of molecules in a solid and time-intervals comparable with what is called the period of an electron.

(3) Between these we have the *meso-domain*, which may also be called 'macrodifferential' or 'physically small.' A meso-volume is one whose linear dimensions are large compared with atomic distances, but small compared with distances within which changes in quantities are discernible by the usual experimental methods. A meso-duration is a time-interval long compared with the period of an orbital electron or the interval

between two impacts of a molecule, but very short from an experimental point of view.

Consider a number of point-charges in a meso-domain (Fig. 44). C is a fixed selected point within the domain; a and r are the macro-distances from O. Since s/r is small, we have the Legendre expansion:

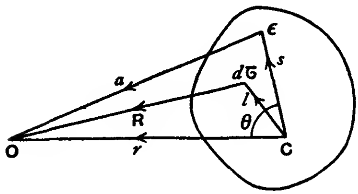

Fig. 44.

$$\frac{1}{a} = [r^2 - 2rs\cos\theta + s^2]^{-\frac{1}{2}}$$

$$= \frac{1}{r} + \frac{s\cos\theta}{r^2} + \frac{s^2}{2r^3}(3\cos^2\theta - 1) + \ldots$$

The potential at O is

$$\varphi = \Sigma e/a = (\Sigma e)/r + (\Sigma es\cos\theta)/r^2 + \ldots$$
$$= (\Sigma e)/r + \{(\Sigma e\mathbf{s})\mathbf{r}\}/r^2 + \ldots \qquad (10.1)$$

Physical considerations show that in practically all cases terms beyond the second are negligible. We now make the assumption that the charges e can be divided into (1) free charges e_1 whose total is $\Sigma e_1 = q$, and (2) bound charges e_2 which are arranged in pairs $\mp e_2$ so that $\Sigma e_2 = 0$ and $\Sigma e_2\mathbf{s}_2 = \mathbf{p}$, the polarisation.[1] Equation (10.1) then becomes

$$\varphi = q/r + (\Sigma e_1\mathbf{s}_1\mathbf{r})/r^3 + (\mathbf{pr})/r^3. \qquad (10.2)$$

Consider now the potential of a continuous distribution of charge and polarisation spread over the same meso-volume (Fig. 44):

$$\psi = \int d\tau\rho/R + \int d\tau(\mathbf{PR})/R^3$$
$$= \int \rho d\tau/r + \int d\tau\rho(\mathbf{Ir})/r^3 + \int d\tau(\mathbf{Pr})/r^3 + \text{higher terms.}$$

We shall have $\psi = \varphi$ provided we take

$$q = \int \rho d\tau$$
$$\Sigma e_1\mathbf{s}_1 = \int \rho d\tau\mathbf{I}$$
$$\mathbf{p} = \int \mathbf{P}d\tau. \qquad (10.3)$$

[1] That this is so can easily be seen by taking any pair $\mp e$, for which $\Sigma es = e\mathbf{s}_1 - e\mathbf{s}_2 = e\mathbf{d}$ where $\mathbf{d} = \mathbf{s}_1 - \mathbf{s}_2$ is the vector drawn from $-e$ to $+e$.

Now it is of far-reaching mathematical convenience to be able to deal with continuous functions. We therefore replace (10.2) by

$$\varphi = \int \rho d\tau/r + \int d\tau(\mathbf{Pr})/r^3, \tag{10.4}$$

where we have now replaced R by r as the current radius-vector. And by extending the region of integration we can apply this expression for the potential to a macroscopic domain.

It is clear that certain physical assumptions underlie this procedure. It is assumed that so far as experimental investigation goes we cannot penetrate a meso-domain—except in so far as regular patterning reveals itself statistically. A meso-volume is a cell of dimensions small relatively to the distance from O, but large enough to contain a great number of charges. And before we reach measurable quantities we have ordinarily to take a large number of such meso-domains into account. We can therefore expect that the statistics will be regular, so that the particular circumstances of individual charges will not influence any measurable quantity and the percentage variation of any such quantity in neighbouring domains will be very small. The reason that polarisation emerges is that it depends on a definite non-random geometrical distribution of charge-pairs. So far as ordinary experiment is concerned we can replace the discontinuities, as in (10.3), by equivalent distributions continuous within the meso-domains.

We have now invented a continuous function φ so as to obtain a convenient method of approximating to the potential at a point O due to distant charge-systems. The value of φ can be calculated at inside points, where ρ differs from zero, since the integral is convergent though improper. But so far we have not assigned any physical significance to this analytical prolongation of the function. So far our analysis applies only to the action of macroscopically distant charge-groups, whereas charges within a body are subject to the action of both far and near charges.

Round a point O $(x_0y_0z_0)$ within a body draw a sphere S', constituting a region of radius a. Let φ' be the potential due to all charges lying outside S' (Fig. 45a). That is

$$\varphi' = \int_{\tau-\tau'} \rho d\tau/r + \int_{\tau-\tau'} \left(\mathbf{P}\nabla \frac{1}{r}\right) d\tau + \int_{S} \sigma dS/r. \tag{10.5}$$

Now $-\nabla_0\varphi'$ is not the intensity $\mathbf{E}'$ due to all the charges outside S', if in calculating the variation of φ' from point to point the sphere S' is carried with the variable point. For in this case

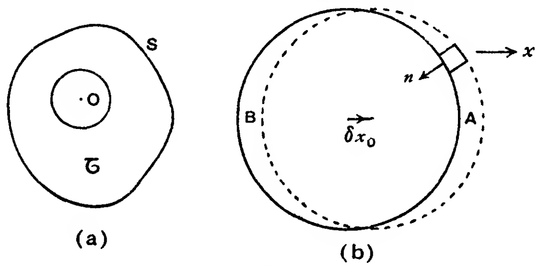

Fig. 45.

certain charges are removed from consideration and other new ones are added. But if by the notation ∇_0' we take account of the variation from point to point within a *fixed* sphere S', we have

$$-\mathbf{E}' = \nabla_0'\varphi'$$
$$= \int\limits_{\tau-\tau'} \rho d\tau \nabla_0 \frac{1}{r} + \int\limits_{\tau-\tau'} d\tau \nabla_0 \left(P\nabla \frac{1}{r}\right) + \int dS\sigma\nabla_0 \frac{1}{r}. \qquad (10.6)$$

Consider the function [1a]

$$\psi = \int\limits_{\tau-\tau'} d\tau f(x_0 y_0 z_0\, xyz).$$

A change corresponding to the increment δx_0 in x_0 consists of two parts: First the parameter x_0 in the integrand becomes $x_0 + \delta x_0$. This gives

$$\delta x_0 \int d\tau \frac{\partial f}{\partial x_0}.$$

Next there is a shift in the sphere S' (Fig. 45 b). Elements of volume $d\tau = dS' \cos(nx)\delta x_0$ are added to the integrand, where $\mathbf{n}$ is the inner normal since the region A is removed from and the region B is added to the field of integration. Hence this part of the change is

$$\delta x_0 \int dS' f \cos(nx).$$

Hence

$$\frac{\partial \psi}{\partial x_0} = \int d\tau \frac{\partial f}{\partial x_0} + \int dS' f \cos(nx) \qquad (10.7)$$

[1a] Mason-Weaver, p. 81.

or

$$\nabla_0 \psi = \nabla_0' \psi + \int dS' \mathbf{n} f.$$

Applying this to φ', we have

$$\nabla_0 \varphi' = \nabla_0' \varphi' + \int dS' \mathbf{n} \left[\rho / r + \left(\mathbf{P} \nabla \frac{1}{r} \right) \right].$$

Let us now take the sphere S' so small that ρ and $\mathbf{P}$ are practically constant within it, having the values appropriate to the point O. There is a certain amount of assumption here, for it means that we take τ' to be a meso-volume and we are using the expression for φ at meso-distances from the elements close to S'. We then have

$$\nabla_0 \varphi' - \nabla_0' \varphi' = \rho_0 / a \,.\int \mathbf{n} dS' - (P_0 / a^2 \,.\int dS' \mathbf{n} \cos \theta),$$

where θ is the angle between $\mathbf{P}_0$ and a line drawn from O to dS'. The first integral is clearly zero, and the second integral has no component perpendicular to $\mathbf{P}_0$. The component parallel to $\mathbf{P}_0$ is

$$\begin{aligned} &+ P_0 / a^2 \,.\int_0^\pi \cos^2 \theta \; 2\pi a^2 \sin \theta d\theta \\ &= 4\pi P_0 / 3, \end{aligned}$$

independently of a. Hence

$$\mathbf{E}' = - \nabla_0 \varphi' + 4\pi \mathbf{P}_0 / 3. \tag{10.8}$$

Since $\mathbf{P}$ is defined only statistically ($\mathbf{p} = \int \mathbf{P} d\tau$) for a meso-volume, so that the polarisation-intensity at the point O has by itself no meaning, it is not easy to justify the foregoing equation on the basis of the electron theory. It can be regarded only as an approximation.

We now proceed to an apparently more hazardous step. Let $\varphi \equiv \lim \varphi'$ as $a \to 0$ be the analytical extension, for points within a body, of the function which at exterior points gives the potential due to the body.

It is easy to see that $\mathbf{E}'$ defined by (10.6) is independent of a, provided ρ and $\mathbf{P}$ are sensibly constant over the sphere S'. For two different values of $\mathbf{E}'$, corresponding to S_1' and S_2' differing in radius, differ by the intensity at the centre due to the uniformly charged and uniformly polarised spherical shell $S_1' - S_2'$; and this is zero. Hence

$$\mathbf{E}' = \lim \mathbf{E}' = - \lim \nabla_0 \varphi' + 4\pi \mathbf{P}_0 / 3. \tag{10.9}$$

Considering only the first integral in the expression (10.5) for φ', we have

$$\frac{\partial\varphi'}{\partial x_0} = \int \rho d\tau \frac{\partial}{\partial x_0}\frac{1}{r} + \int dS'\rho \cos(nx) \, . \, /r.$$

This last integral is zero when ρ is constant. We can deal similarly with the second integral in (10.5); the third integral is regular and independent of a.

Hence

$$\lim \partial\dot{\varphi}'/\partial x_0 = \partial\varphi/\partial x_0.$$

Dropping the zero subscript in (10.9), we therefore have

$$\begin{aligned} \mathbf{E}' &= -\nabla\varphi + 4\pi\mathbf{P}/3 \\ &= \mathbf{E} + 4\pi\mathbf{P}/3 \\ &= \mathbf{D} - 8\pi\mathbf{P}/3. \end{aligned} \tag{10.10}$$

Here $\mathbf{E}$ stands for the analytical expression $-\nabla\varphi$, and $\mathbf{D}$ for $\mathbf{E} + 4\pi\mathbf{P}$. This equation is of course identical with (2.5) at which we arrived by the ordinary continuous analysis.

Note that it is for purely mathematical reasons that we have substituted (10.10) for (10.8), i.e. that we have taken $a \to 0$. In (10.10) $\mathbf{E}'$ still denotes the force per unit charge inside a meso-sphere whose other contained charges are *not* taken into account. The contribution of these latter is zero for a cubic crystal and also approximately for a fluid with a random distribution of molecules, provided we assume that the electric moment of a molecule retains its average value through all the phases of thermal motion. But even in this case we still have a force exerted by charges within a micro-distance. In the case of cubic crystals, and approximately for liquids and isotropic solids, we can take the charges lying in a spherical shell, bounded exteriorly by the meso-radius a and interiorly by a radius comparable with intermolecular distances, as statistically symmetrical, i.e. as equivalent to a continuous distribution of density and polarisation. There remains the effect of the neighbouring charges. In the case of a polarised dielectric in equilibrium, these charges must produce a force $\mathbf{F}$ which balances $\mathbf{E}'$:

$$\mathbf{F} = -\mathbf{E}' = -\mathbf{E} - 4\pi\mathbf{P}/3. \tag{10.10a}$$

It is generally assumed that $\mathbf{F}$ is proportional to $\mathbf{P}$ or, what comes to the same thing, that $\mathbf{P}$ is proportional to $\mathbf{E}$:

$$\begin{aligned} \mathbf{P} &= (\varkappa - 1)/4\pi \, . \, \mathbf{E} \\ \mathbf{F} &= -(\varkappa + 2)/3 \, . \, \mathbf{E}. \end{aligned}$$

Or, putting the assumption in another way, for a substance with no permanent doublets and with n molecules per unit volume :

$$\begin{aligned}\mathbf{P} &= -n\alpha\mathbf{F}\\ &= n\alpha(\mathbf{E} + 4\pi\mathbf{P}/3).\end{aligned}$$

Whence

$$(\varkappa - 1)/(\varkappa + 2) = 4\pi n\alpha/3.$$

If w is the molecular weight, ρ the density, m the mass of a molecule, N Avogadro's number, $w = mN$ and $\rho = mn$. Therefore [2]

$$w(\varkappa - 1)/\rho(\varkappa + 2) = 4\pi N\alpha/3. \qquad (10.10b)$$

When equilibrium does not exist we know that the bound electrons in a molecule execute vibrations. We therefore assume that the neighbouring charges exert at any moment on the electron a restoring force proportional to its displacement ($\mathbf{r}$) from its equilibrium position ; and we shall afterwards add a damping force. The force will thus be of the form

$$e\mathbf{E}' - m\omega_0^2\mathbf{r} - mh\dot{\mathbf{r}}, \qquad (10.11)$$

and the polarisation-intensity will be $\mathbf{P} = Ne\mathbf{r}$.

We shall now briefly glance at a few typical statements which display considerable embarrassment.

> It is desirable to know the effective average field to which a molecule is subjected when a macroscopic field **E** is applied. The effective field is not the same, even in the mean, as the macroscopic **E**, despite the fact that the vector **E** is the space average of the microscopic field **e** over a physically small volume-element. The explanation of this paradox is that the effective field in which we are interested is that in the interior of a molecule, whereas the space averaging presupposed in the relation **E** = average **e** is over regions both exterior and interior to molecules. The effective field within a molecule may be resolved into two parts : first, the internal field exerted by other charges within the same molecule ; and second, the remainder due not only to the applied electric field but also to the attractions and repulsions by other molecules, usually polarised under the influence of the external field [i.e. **E′**].—J. Van Vleck, p. 14.
>
> [**E′** is the] average local field acting in the interior of a molecule. . . . This average is not in general equal to the field entering into the macroscopic field equations, since **E** is obtained by averaging the local field in a different way—for example, over a physically small volume-element still large enough to contain many molecules.—Kirkwood, p. 592.

[2] H. A. Lorentz, AP 9 (1880) 642 ; L. Lorenz, AP 11 (1880) 77.

Certainly there is a 'paradox' in this enunciation of two different 'averages,' and their attempted reconciliation is anything but clear. The confusion is entirely due to Lorentz's erroneous statement —which we shall presently criticise—that $\mathbf{E}$, i.e. the analytical function $-\nabla\varphi$, is a macroscopic average, whatever that means. Writers who accept Lorentz's view are compelled to resort to curious subtleties.

> If we bring the introduced charge e to all possible points of a physically small volume-element, measure the force each time and then find the average value of these measures, this average value is $e\mathbf{E}$. In the present problem there is question of the force $e\mathbf{E}'$ which a charge e belonging to the body itself experiences. This charge itself contributes to $\mathbf{E}$; but we first prescind from this part of $\mathbf{E}$ and calculate the force of all *other* charges of the body on e.—Försterling, p. 88.

This author then proves (10.10) and never returns to the problem from which he has prescinded or abstracted. The truth is that this distinction between an introduced or alien charge and one which belongs to the body is excogitated only in order to try to reconcile with formula (10.10) Lorentz's view of $\mathbf{E}$ as an 'average.'

Frenkel's treatment of 'the average field of a single neutral molecule' is as follows (ii. 68):

> We imagine such a molecule as a small sphere K of radius a, inside of which the bound electrons may be arranged in any way. As regards the calculation of the *external* field produced by the molecule, this arrangement can be replaced by any other distribution of charge and current inside or *upon the surface* of K, provided this gives rise to the same values of the electric and magnetic moments of the molecule. We shall now assume that, in calculating the *average* field *inside* the molecule (i.e. inside the sphere K), all *such equivalent electricity-distributions give the same result.* Of course this assumption cannot be strictly justified; yet it must certainly apply as a first approximation. So, instead of the actual arrangement of the electrons, we imagine the equivalent distribution on the surface of K, and we calculate the spatial average value of the electric and magnetic intensities inside a larger sphere S (radius l) concentric with K. . . . If there are N molecules per unit volume, $\tau \equiv 4\pi l^3/3 = 1/N$.

The electrostatic field due to the molecule (average moment $\mathbf{M}$) has outside

$$\text{potential } \varphi = (\mathbf{Mr})r^{-3}$$

$$\text{force } \mathbf{F} = 3\mathbf{r}(\mathbf{Mr})r^{-5} - \mathbf{M}r^{-3}$$

and inside

$$\text{potential } \varphi' = (\mathbf{Mr})a^{-3}$$
$$\text{force } \mathbf{F}' = -\mathbf{M}a^{-3}.$$

If $\mathbf{F}_m$ is the average value (at the centre)

$$\tau\mathbf{F}_m = \int_0^a \mathbf{F}'d\tau + \int_a^l \mathbf{F}d\tau.$$

'It is now easy to see that the [second] integral vanishes, for the average value of $\mathbf{F}$ for different directions of $\mathbf{r}$ (with constant r) is zero.' Hence

$$\mathbf{F}_m = -4\pi N\mathbf{M}/3$$
$$= -4\pi\mathbf{P}/3.$$

> We are now in a position to calculate the external or effective field-strength acting on a molecule. This effective field [$\mathbf{E}'$] is found by subtracting the field generated by the molecule itself [$\mathbf{F}_m$] from the average total field ($\mathbf{E}$).

Hence

$$\mathbf{E}' = \mathbf{E} - (-4\pi\mathbf{P}/3).$$

This is a singular argument; it ends by professing to give the force on a molecule, whereas what we require is the force (10.10) on an electron. Apparently we are asked to believe that a polarised molecule exerts (on what ?) an average force $-4\pi\mathbf{P}/3$ at its own centre. And this 'average' is calculated by replacing the molecule by a spherical surface-distribution of doublets, and then—for no assigned reason—taking the volume-integral of the force. It is a mere mathematical *tour de force* without physical relevance.

2. The Vector Potential.

Let us now investigate the vector potential due to a meso-complex of moving charges. In Fig. 44 the 'centre' C is at rest so that $\dot{\mathbf{r}} = 0$ and the velocity of e is $\mathbf{v} = \dot{\mathbf{s}}$. We have

$$c\mathbf{A} = \Sigma(e\mathbf{v}/a) = (\Sigma e\mathbf{v})/r + \Sigma e\mathbf{v}(\mathbf{rs})/r^3 + \ldots \quad (10.12)$$

Dividing the charges into free and bound (doublets) as before, we have

$$\Sigma e\mathbf{v} = \Sigma e_1\dot{\mathbf{s}}_1 + \Sigma e_2\dot{\mathbf{s}}_2.$$

The second term is the rate of change of $\Sigma e_2 \mathbf{s}_2 = \mathbf{p}$. As before we put

$$\mathbf{p} = \int \mathbf{P} d\tau.$$

Similarly the first term, representing the ordinary current, is thus expressed:

$$\Sigma e_1 \dot{\mathbf{s}}_1 = \int \mathbf{u} d\tau.$$

The total current is

$$\Sigma e \mathbf{v} = \int (\mathbf{u} + \dot{\mathbf{P}}) d\tau. \qquad (10.13)$$

Consider next the second term in (10.12), higher terms being neglected. Since

$$\frac{\partial}{\partial t} \mathbf{s}(\mathbf{rs}) = \mathbf{v}(\mathbf{rs}) + \mathbf{s}(\mathbf{rv}),$$

$$V\mathbf{r}V\mathbf{sv} = \mathbf{s}(\mathbf{rv}) - \mathbf{v}(\mathbf{rs}),$$

$$\mathbf{v}(\mathbf{rs}) = \tfrac{1}{2} \frac{\partial}{\partial t} \mathbf{s}(\mathbf{rs}) - \tfrac{1}{2} V\mathbf{r}V\mathbf{sv}.$$

Hence

$$\Sigma e \mathbf{v}(\mathbf{rs}) = \tfrac{1}{2} \frac{\partial}{\partial t} \Sigma e \mathbf{s}(\mathbf{rs}) - cV\mathbf{r}\mathbf{M},$$

where
$$\mathbf{M} = \Sigma e V\mathbf{sv}/2c.$$

The first term, being a time-variation, we take to be statistically zero; it is clearly zero for a uniform drift, for a random distribution of velocities, and for any periodic motion (the time-average being taken over an interval very great compared with a period). The second term we treat as follows

$$\Sigma e V\mathbf{sv}/2c = \mathbf{M} = \int \mathbf{I} d\tau. \qquad (10.14)$$

On substituting these continuous integrals which may be extended over a macroscopic volume, we obtain

$$\mathbf{A} = \int d\tau (\mathbf{u} + \dot{\mathbf{P}})/cR + \int d\tau V\mathbf{I}\nabla\frac{1}{R}.$$

Changing the notation by writing r for R and putting $\mathbf{w} = \mathbf{u} + \dot{\mathbf{P}}$, we have, exactly as for (2.9 and 10),

$$A = \int d\tau \mathbf{w}/cr + \int d\tau \text{ curl } \mathbf{I} \,.\, /r. \qquad (10.15)$$

in which the second integral may be regarded as including $\int dS$ curls $\mathbf{I}\,.\,/r$ as a limiting case. We have thus arrived at the total vector potential as the sum of

$$\text{the electric vector potential } \mathbf{A}_1 = \int d\tau \mathbf{w}/cr$$

$$\text{and the magnetic vector potential } \mathbf{A}_2 = \int d\tau \text{ curl } \mathbf{I}\,.\,/r.$$

It is clear that we are justified in calling $\mathbf{M}$ the magnetic moment of the complex, for the vector potential of a magnetic doublet is

$$-\,V\mathbf{M}\nabla\frac{1}{r} = V\mathbf{M}\mathbf{r}/r^3.$$

The areal velocity of a single point-charge is

$$\tfrac{1}{2}V\mathbf{r}\mathbf{v} = \mathbf{n}S/T, \tag{10.16}$$

where $\mathbf{r}$ is the radius-vector drawn from the force-centre, $\mathbf{n}$ is unit normal to the orbital plane, S is the area of the orbit and T is the period. The effective current is $j = e/T$, and it acts as a small magnet of moment (4.8a)

$$\mathbf{M} = j\mathbf{n}S/c = e/2c\,.\,V\mathbf{r}\mathbf{v}$$

in mag units. Since the angular momentum round the force-centre is

$$\mathbf{P} = mV\mathbf{r}\mathbf{v},$$

the ratio of these two quantities is for an electron

$$\begin{aligned} R \equiv P/M &= 2mc/e \\ &= -\,1{\cdot}13 \times 10^{-7}. \end{aligned} \tag{10.16a}$$

If the fixed point C is vectorially distant b from the force-centre, $\mathbf{s} = \mathbf{b} + \mathbf{r}$ and $\mathbf{v} = \dot{\mathbf{s}} = \dot{\mathbf{r}}$. Then

$$V\mathbf{s}\mathbf{v} = \partial/\partial t\,.\,V\mathbf{b}\mathbf{r} + V\mathbf{r}\mathbf{v},$$

so that on the average

$$\Sigma eV\mathbf{s}\mathbf{v} = \Sigma eV\mathbf{r}\mathbf{v}. \tag{10.16b}$$

In the present analysis the subdivision of the vector potential is regarded merely as the result of the respective magnitude of the dimensions involved; what we call magnetism is due to differential motion within a meso-volume. But we have not thereby ousted Poisson's analysis; on the contrary we have

justified it, as can be seen by comparing the second integral of (10.15) with formula (2.10a).

It is worth while verifying the expressions for the force and torque on the meso-complex of charges, due to an external magnetic field whose value at the centre C is $\mathbf{H}$. The force is

$$\begin{aligned}\mathbf{F} &= c^{-1}\Sigma V[e\mathbf{v},\, \mathbf{H}+(\mathbf{s}\nabla)\mathbf{H}] \\ &= c^{-1}V\mathbf{wH}+c^{-1}\Sigma V[e\mathbf{v},\,(\mathbf{s}\nabla)\mathbf{H}].\end{aligned}$$

The x-component of the second term is

$$c^{-1}\Sigma ev_y(\mathbf{s}\nabla H_z)-c^{-1}\Sigma ev_z(\mathbf{s}\nabla H_y).$$

Now we have already shown that

$$c^{-1}\Sigma e\mathbf{v}(\mathbf{sr}) = -\,V\mathbf{rM}. \tag{10.17}$$

Substituting $\mathbf{q}$ for $\mathbf{r}$, the y-component of this is

$$c^{-1}\Sigma ev_y(\mathbf{sq}) = q_x M_z - q_z M_x.$$

Putting $\mathbf{q} = \nabla H_z$ and similarly putting $\mathbf{q} = \nabla H_y$ in the z-component, we obtain for the x-component of the second term in $\mathbf{F}$:

$$-\,M_x\left(\frac{\partial H_y}{\partial y}+\frac{\partial H_z}{\partial z}\right)+M_y\frac{\partial H_y}{\partial x}+M_z\frac{\partial H_z}{\partial x} = \Sigma M_x\frac{\partial H_x}{\partial x},$$

since div $\mathbf{H}$ = curl $\mathbf{H} = 0$. Hence

$$\mathbf{F} = c^{-1}V\mathbf{wH}+(\mathbf{M}\nabla)\mathbf{H}. \tag{10.18}$$

Similarly the torque is

$$\mathbf{G} = \Sigma V[\mathbf{s},\, e/c\,.\,V\mathbf{vH}],$$

where it is a legitimate approximation, owing to the presence of the factor s, to take $\mathbf{H}$ the value of the field at C. Now substituting $\mathbf{H}$ for $\mathbf{r}$ in (10.17), we have

$$c^{-1}\Sigma e\mathbf{v}(\mathbf{sH}) = -\,V\mathbf{HM} = V\mathbf{MH}.$$

Hence

$$\begin{aligned}\mathbf{G} &= c^{-1}\Sigma eV\mathbf{s}V\mathbf{vH} \\ &= c^{-1}\Sigma e\mathbf{v}(\mathbf{sH})-c^{-1}\Sigma e\mathbf{H}(\mathbf{sv}) \\ &= V\mathbf{MH}-\mathbf{H}/2c\,.\,\frac{\partial}{\partial t}\,\Sigma es^2 \\ &= V\mathbf{MH},\end{aligned} \tag{10.18a}$$

since the average value of the second term is zero. We have therefore verified that, as regards force and torque, $\mathbf{M}$ behaves as a magnetic doublet.

Regarding magnetism as due to electrons in rotational or orbital motion, and prescinding from free or polarised charges, we can

utilise an analogy which is here referred to because it has been misconstrued. In the case of polarising electrons

$$\varphi = \int d\tau(\mathbf{Pr})/r^3 = \int d\tau\left(\mathbf{P}\nabla\frac{1}{r}\right)$$

where $\mathbf{P} = \int \rho\mathbf{s}d\tau$. We know, by means of the simple formula

$$\operatorname{div}(\mathbf{P}/r) = \frac{1}{r}\operatorname{div}\mathbf{P} + \left(\mathbf{P}\nabla\frac{1}{r}\right)$$

and Green's theorem, that the polarised body can be viewed as a non-polarised body having a volume-density ρ', the surface-density being regarded as a limiting case. That is, $\varphi = \int \rho' d\tau/r$, where $\rho' = -\operatorname{div}\mathbf{P}$. In the case of magnetising electrons,

$$\begin{aligned} A_x &= (\Sigma ev_x\mathbf{s}\,.\,\mathbf{r})/cr^3 \\ &= (\mathbf{qr})/r^3, \text{ where } \mathbf{q} \text{ is } \Sigma ev_x\mathbf{s}/c \\ &= \int d\tau(\mathbf{Qr})/r^3, \end{aligned}$$

where

$$\mathbf{Q} = \int d\tau\rho v_x\mathbf{s}/c.$$

By a simple mathematical analogy, without physical significance, we infer that

$$A_x = \int u_x d\tau/cr,$$

where

$$u_x/c = -\operatorname{div}\mathbf{Q}.$$

Since there is no charge or polarisation, we have $\Sigma e = \Sigma e\mathbf{s} = 0$ over a meso-volume. Also for the type of steady motion here envisaged we can take Σex^2, Σeyz, etc., as independent of the time, (xyz) being the vector $\mathbf{s}$. Hence

$$\Sigma exv_x = \Sigma eyv_z + \Sigma ezv_y = \text{etc.} = 0.$$

Utilising this result, we have

$$\begin{aligned} -c\operatorname{div}\mathbf{q} &= -\frac{\partial}{\partial x}\Sigma ev_x x - \frac{\partial}{\partial y}\Sigma ev_x y - \frac{\partial}{\partial z}\Sigma ev_x z \\ &= \tfrac{1}{2}\frac{\partial}{\partial y}\Sigma(exv_y - eyv_x) - \tfrac{1}{2}\frac{\partial}{\partial z}\Sigma(ezv_x - exv_z) \\ &= c\,(\operatorname{curl}\mathbf{M})_x. \end{aligned}$$

Whence

$$\mathbf{A} = \int \mathbf{u} d\tau / cr,$$

where $\mathbf{u}/c = \text{curl } \mathbf{I}$.

That is, in accordance with (10.15), the magnetising electrons may be regarded as contributing a current c curl $\mathbf{I}$. The former proof is obviously more simple and direct. The present analogical proof contains nothing more subtle than an application of Green's theorem. Hence the following attempt to glorify it into a general statistical theorem applicable to 'regions taken at random' must be rejected :

> If over each molecule or group of electrons $\int \rho d\tau = 0$, then over a region, taken at random and large enough to contain a large number of molecules or groups, the charge per unit volume is $-\text{div } \mathbf{P}$, where $\mathbf{P}$ is the mean value per unit volume of $\int \rho \mathbf{r} d\tau$. It is clear that if the boundary were drawn deliberately with infinite precision, in such a manner as never to cut through any group and so as to contain only entire groups, the total charge and so the density would be zero. But when we speak of $-\text{div } \mathbf{P}$ as the density, we mean the density in any element of volume taken at random. Regarding this as a purely analytical theorem, its application may be generalised in the following manner. . . . —G. T. Walker, ii. 36.

Formula (10.15) can be written

$$\mathbf{A} = \int d\tau \mathbf{U} / cr,$$

where $\mathbf{U} = \mathbf{u} + \dot{\mathbf{P}} + c \text{ curl } \mathbf{I}$.

Hence in a certain sense we can say that we have substituted $\mathbf{U}$ for $\mathbf{u}$ to express the current-intensity. But we must be careful not to misinterpret this statement. Our argument can be divided into three stages :

(1) $\mathbf{A} = \Sigma(e\mathbf{v}/ca)$.

(2) $\mathbf{A} = (\Sigma e_1 \mathbf{v}_1 + \dot{\mathbf{p}})/cr + V\mathbf{M}\mathbf{r}/r^3$.

(3) $\mathbf{A} = \int d\tau(\mathbf{u} + \dot{\mathbf{P}})/cR + \int d\tau \text{ curl } \mathbf{I} \,.\, /R$.

(1) The first is the primary formula supposed to be microscopically valid at least as regards the statistical results it produces. Neither polarisation nor magnetisation occurs.

(2) The second stage is introduced to cope with the experi-

mental fact that quantities within a meso-domain, spatial and temporal, are indiscernible; we cannot distinguish between lengths such as r and a, nor can we deal with time-intervals less than a meso-duration. The emergence of the quantities $\mathbf{p}$ and $\mathbf{M}$ is due to their statistical persistence caused by certain peculiarities of configuration which exist in certain cases.

(3) The third stage is devoid of physical significance; it is adopted purely for convenience of mathematical manipulation.

Observe that if we omitted the second step altogether and put

$$\Sigma(e\mathbf{v}/a) = \int \mathbf{u}d\tau/R,$$

we should have neither polarisation nor magnetisation. The question of generalising $\mathbf{u}$ into $\mathbf{U}$ would not arise at all. It follows that the generalisation represented by (10.15) is not any physical hypothesis concerning the nature of 'current' in certain circumstances. It is merely the expression, clothed artificially in the language of the calculus, of our practical inability to deal with the individual peculiarities of a meso-domain. It would accordingly be ridiculous to regard (10.15) as having any significance for ultimate physical theory.

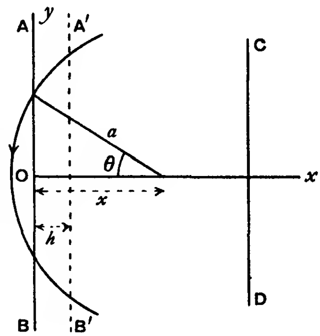

Fig. 46.

We have developed our argument in terms of the vector potential. But it will be instructive to work it out in terms of the current which for a meso-volume we define as the aggregate $\Sigma e\mathbf{v}$. This consists of three contributions:

(1) The free charges give $\Sigma e_1\mathbf{v}_1$.
(2) The polarisation charges give $\Sigma e_2\mathbf{v}_2 = \dot{\mathbf{p}}$.
(3) The magnetisation electrons make an average contribution which must now be investigated.[3]

Consider the electrons as moving in circular orbits of radius a. Divide the meso-volume by planes parallel to yz at distances a, such as AB and CD in Fig. 46. Let $A'B'$ be a plane close to AB,

[3] Försterling, p. 599.

at a distance h from it. Let us investigate the contribution to the y-current due to orbits in planes xy. Each circle cuts the plane AB twice; we can omit the rare case of a centre lying on one of the planes. The revolving electron is for only a fraction of its orbit within the space between unit areas on AB and $A'B'$, and on the average contributes

$$2(eds/2\pi a)v_y$$

to the y-component of the current. We may assume that the number of centres in unit volume between $x=0$ and $x=x$ is $N=N_0+nx$, where $n=\partial N/\partial x$. The contribution of the orbits with centres between x and $x+dx$ and those with centres between $-x$ and $-(x+dx)$ is

$$edsv_y/\pi a\,.\,[N_0+nx-(N_0-nx)]dx.$$

Hence the total contribution of circles cutting AB is

$$\Sigma_{x=0}^{x=a}\ 2edsv_y/\pi a\,.\,nxdx.$$

Now $x=a\cos\theta$, $dx=-a\sin\theta d\theta$, $ds=h/\sin\theta$, $v_y=-v\cos\theta$. Hence the result is

$$-\frac{2evah}{\pi}\frac{\partial N}{\partial x}\int_0^{\pi/2}\cos^2\theta d\theta$$
$$=-\frac{eav}{2}\frac{\partial N}{\partial x}h.$$

The magnetic moment of each orbit is $eav/2c$ along the z-axis. Putting $M_z=Nheav/2c$, remembering that h is the volume, we have as the y-current

$$-c\partial M_z/\partial x$$

Similarly the orbits with centres on the 3-axis contribute

$$+c\partial M_x/\partial x.$$

That is, the current due to the magnetising electrons is c curl $\mathbf{M}$.

Having found the total current to be

$$\Sigma e_1\mathbf{v}_1+\dot{\mathbf{p}}+c\,\text{curl}\,\mathbf{M},$$

we still have to make the transition to the integral form $\int\mathbf{U}d\tau$ and the formula $c\mathbf{A}=\int\mathbf{U}d\tau/r$, where

$$\mathbf{U}=\mathbf{u}+\dot{\mathbf{P}}+c\,\text{curl}\,\mathbf{I}.$$

To do this we have to adopt exactly the same reasoning as before. Our analysis is always based on the indistinguishability of indi-

vidual elements within a meso-volume and the emergence of separate terms due to statistical regularity (doublet-configuration or orbital motions).

In connection with this argument we must avoid making undue claims. (1) It is not true that the formulae, as above developed, have been verified experimentally. (2) It is not true that our sole premisses are the scalar potential of electrostatics, the vector potential of electrodynamic experiments, and the idea of statistical analysis. For, as we shall see later, we have also covertly assumed that the fundamental law of force between moving charges involves their separate absolute velocities (*i.e.* their velocities relative to the aether) and not merely their velocity relative to one another. It is therefore advisable to give the argument, in so far as it holds, in a simpler form which is closer to actual experiment and which does not import the idea of absolute velocity.

We start with closed neutral uniform currents and find (4.9) the vector potential

$$\mathbf{A}_1 = \int \mathbf{j} ds/cr,$$

which we can transform into

$$\mathbf{A}_1 = \int \mathbf{u} d\tau/cr,$$

provided div $\mathbf{u} = 0$. The use of the integral calculus already implies that we can only deal with statistical results. We next assume that magnetism is the result of the existence of a great number of neutral microscopic circuits, each of which, in accordance with (4.8a), acts as a small magnetic doublet. Once more we replace summation by integration and take the total moment to be $\int \mathbf{I} d\tau$. In accordance with (2.9 and 10) this gives the vector potential

$$\mathbf{A}_2 = \int d\tau V \mathbf{I} \nabla \frac{1}{r} = \int d\tau \text{ curl } \mathbf{I} \,.\, /r.$$

The total vector potential is therefore

$$\mathbf{A} = \int d\tau (\mathbf{u}/c + \text{curl } \mathbf{I})/r, \tag{10.19}$$

in which the term $\dot{\mathbf{P}}$ is missing as the currents are steady. This simple argument avoids doubtful assumptions and is kept in close contact with experiment. We have given the more elaborate

and less certain analysis, because the text-books are—unwittingly, it is to be presumed—based entirely on the Liénard force-law as if it were beyond criticism or replacement ; and also it seems to be *de rigueur* to deduce Maxwell's equations for a material medium.

Taking O to be an external point, we have from (1.3), since $\mathrm{curl}_0\, \mathbf{u} = 0$ as $\mathbf{u}$ is a function of (xyz),

$$\mathrm{curl}_0\, (p\mathbf{u}) = V(\nabla_0 p, \mathbf{u}) = V\mathbf{u}\nabla p,$$

where p stands for $1/r$. Hence

$$\mathrm{curl}_0 \int d\tau p\mathbf{u} = \int d\tau V\mathbf{u}\nabla p.$$

Therefore

$$\begin{aligned} \mathbf{H} &\equiv \mathrm{curl}_0\, \mathbf{A} \\ &= \int d\tau V[\mathbf{w}/c + \mathrm{curl}\, \mathbf{I}, \nabla p]. \end{aligned} \tag{10.20}$$

As a particular case, this gives for linear circuits in vacuum

$$\mathbf{H} = J \int V d\mathbf{s}\nabla p.$$

From (1.5), since $\mathrm{curl}_0\, \mathbf{I} = 0$ and $\mathrm{curl}_0\, \nabla_0 p = 0$,

$$\mathrm{div}_0\, V\mathbf{I}\nabla p = -\, \mathrm{div}_0\, V\mathbf{I}\nabla_0 p = 0.$$

Also from (1.4)

$$\begin{aligned} \mathrm{div}_0\, (p\mathbf{w}) &= p\, \mathrm{div}_0\, \mathbf{w} + (\mathbf{w}\nabla_0 p) \\ &= -\, (\mathbf{w}\nabla p) \\ &= -\, \mathrm{div}\, (p\mathbf{w}), \end{aligned}$$

since $\mathrm{div}\, \mathbf{w} = 0$ in the steady state. This implies that $\dot{\mathbf{P}} = 0$ and $\mathbf{w} = \mathbf{u}$. Hence

$$\begin{aligned} \mathrm{div}_0\, \mathbf{A} &= -\int d\tau\, \mathrm{div}\, (p\mathbf{w}) = -\int dSpw_n \\ &= 0, \end{aligned} \tag{10.21}$$

for, since the integration is extended over the whole of the current-carrying body, there is no normal flow over the boundary.

Also

$$\begin{aligned} \nabla_0^2\mathbf{A} &= \int d\tau\mathbf{w}\nabla_0^2 p + \int d\tau VI\nabla\nabla_0^2 p \\ &= 0, \end{aligned} \tag{10.22}$$

and

$$\begin{aligned}\operatorname{curl}_0 \mathbf{H} &= \operatorname{curl}_0^2 \mathbf{A} \\ &= -\nabla_0^2 A + \nabla_0 \operatorname{div}_0 A \\ &= 0. \end{aligned} \tag{10.23}$$

These are the ordinary results for the non-retarded vector potential; they are repeated here merely for completeness.

So far we have been dealing with the vector potential at exterior points. Just as we did for the scalar potential, we now proceed to extend analytically, beyond its original region of physical significance, the formula from which $\mathbf{A}$ is computed at distant points.[4] Surround an interior point O by a sphere S' (of radius a). Let $\mathbf{H}'$ be the magnetic intensity and $\mathbf{A}'$ the vector potential at O, due to the charges outside S'. Then, with the same notation as was used in the previous section,

$$\operatorname{div}_0' \mathbf{A}' = 0,$$
$$\operatorname{curl}_0' \mathbf{A}' = \mathbf{H}',$$

where the primed operators denote variation within a fixed sphere S'. Using unprimed operators to denote variation when the deleted sphere is carried with the variable point, we have from (10.7)

$$\operatorname{div}_0 \mathbf{A}' = \operatorname{div}_0' \mathbf{A}' + \int dS' \left[\frac{\mathbf{u}}{cr} + V\mathbf{I}\nabla\frac{1}{r}\right]_n.$$

Subject to the reservations already made, we now take the current-vector $\mathbf{u}$ as sensibly constant over the interior of the sphere. The integral is then zero, for $a^{-1}\int u_n dS' = 0$ in the steady state and $V\mathbf{I}\nabla\frac{1}{r}$ has no component normal to the sphere. Hence

$$\operatorname{div}_0 \mathbf{A}' = \operatorname{div}_0' \mathbf{A}' = 0.$$

Applying (10.7) again, we have

$$\operatorname{curl}_0 \mathbf{A}' = \operatorname{curl}_0' \mathbf{A}' + \int dS' V\mathbf{n}\left[\frac{\mathbf{u}}{cr} + V\mathbf{I}\nabla\frac{1}{r}\right],$$

where $\mathbf{n} = \mathbf{r}_1$ is unit inward normal to the sphere so that $\nabla\frac{1}{r} = -\mathbf{r}_1/r^2$. Since the first part of the integral containing $\mathbf{u}$

[4] Mason-Weaver, p. 201.

(assumed constant) clearly vanishes, and $dS' = 2\pi a^2 \sin\theta d\theta$, and $\nabla(1/r) = \mathbf{r}_1/r^2$, the integral is

$$V = \int dS' V \mathbf{I}\mathbf{r}_1/a^2 = 2\pi \int (\mathbf{I} - \mathbf{r}_1 I_r) \sin\theta d\theta.$$

I, assumed constant, can be taken along the axis from which θ is measured; the components of $\mathbf{r}_1$ are $(-\cos\theta, -\sin\theta)$ and $I_r = -I\cos\theta$ (Fig. 47). Then

$$V_y = -2\pi I \int_0^{2\pi} \sin^2\theta \cos\theta d\theta = 0$$

$$V_x = 2\pi I \int_0^{2\pi} (1 - \cos^2\theta) \sin\theta d\theta = 8\pi I/3.$$

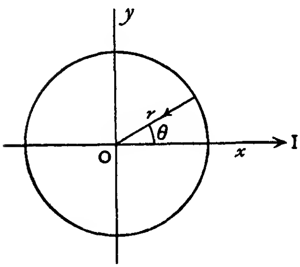

Fig. 47.

Hence

$$\operatorname{curl}_0 \mathbf{A}' = \operatorname{curl}_0' A' + 8\pi\mathbf{I}/3$$

or

$$\mathbf{H}' = \operatorname{curl}_0 \mathbf{A}' - 8\pi\mathbf{I}/3.$$

Let **A** denote lim **A**′ as $a \to 0$, i.e. the analytical extension of the vector potential within the body. Here, as in the case of the scalar potential, it is easy to show that the operators div and curl can be permuted with lim. It is also easy to see that **H**′ is independent of a. Hence

$$\operatorname{div}_0 \mathbf{A} = 0$$
$$\mathbf{H}' = \operatorname{curl}_0 \mathbf{A} - 8\pi\mathbf{I}/3. \qquad (10.24)$$

Defining **B** as $\operatorname{curl}_0 \mathbf{A}$ at interior points, we have

$$\mathbf{H}' = \mathbf{B} - 8\pi\mathbf{I}/3. \qquad (10.25)$$

Comparing this with (10.10), Mason and Weaver declare that '$-\mathbf{I}$, and not **I**, is actually analogous to **P**' and hence 'diamagnetic bodies are analogous to dielectrics' (pp. 215, 221). That this is a complete misinterpretation can be shown at once by defining $\mathbf{H} = \mathbf{B} - 4\pi\mathbf{I}$, when (10.25) becomes

$$\mathbf{H}' = \mathbf{H} + 4\pi\mathbf{I}/3, \qquad (10.26)$$

which exactly corresponds to (10.10). The apparent difference in derivation lies in the fact that in the electrical case we worked with the scalar potential, defining **E** as $-\nabla\varphi$ whether inside or outside the body, whereas in the magnetic case we utilised the

vector potential. But we could also have worked with the scalar potential[5] defined by (2.3):

$$\varphi' = \int d\tau(\mathbf{I}\nabla p),$$

where p stands for $1/r$. Then by (2.9), the vector potential due to 'magnetism' alone is

$$\mathbf{A} = \text{curl} \int d\tau p \mathbf{I},$$

and by (2.11)

$$\begin{aligned} \mathbf{B} &\equiv -\nabla\varphi' + 4\pi\mathbf{I} \\ &= \text{curl } \mathbf{A}. \end{aligned}$$

Hence we could have proved (10.26) exactly as we proved (10.10), and Poisson's analysis is once more justified.

The integrals in the expression (10.15) are improper at interior points. But, exactly as for the scalar potential, we obtain

$$\nabla^2\mathbf{A} = -4\pi(\mathbf{u}/c + \text{curl } \mathbf{I}),$$

where we have dropped the zero suffix. Since div $\mathbf{A} = 0$ (10.24),

$$\text{curl } \mathbf{B} = \text{curl}^2 \mathbf{A} = -\nabla^2\mathbf{A}.$$

Hence

$$\text{curl } \mathbf{H} = 4\pi\mathbf{u}/c, \tag{10.27}$$

which is the same as equation (4.2a) or (5.17).

Dividing the vector potential into $\mathbf{A}_1 = \int \mathbf{u} d\tau/cr$ due to the ordinary currents and $\mathbf{A}_2 = \text{curl} \int \mathbf{I} d\tau/r$ due to the Amperian currents, we have

$$\begin{aligned} \mathbf{H} &= \text{curl } \mathbf{A}_1, \\ \mathbf{H}_2 &= \text{curl } \mathbf{A}_2 = 4\pi\mathbf{I}, \end{aligned}$$

the latter equation being (5.9) with $-\nabla\varphi'$ omitted since all 'magnetism' is included in the micro-circuits. Hence, if the total $\mathbf{A} = \mathbf{A}_1 + \mathbf{A}_2$ and if $\mathbf{B}$ is defined to be $\mathbf{H} + \mathbf{H}_2$,

$$\mathbf{B} = \text{curl } \mathbf{A} = \mathbf{H} + 4\pi\mathbf{I}.$$

[5] We here omit consideration of the current $\mathbf{u}$. In order to take it into account, we should have to take $\mathbf{H} = 4\pi\mathbf{J} - \nabla\phi'$, where curl $\mathbf{J} = \mathbf{u}/c$. Then the total potential is $\mathbf{A} = \text{curl}\int d\tau p(\mathbf{I} + \mathbf{J})$. And $\mathbf{B} \equiv -\nabla\phi' + 4\pi(\mathbf{I} + \mathbf{J}) = \mathbf{H} + 4\pi\mathbf{I}$ $=$ curl $\mathbf{A}$. Also $\mathbf{H} = 4\pi$ curl $\mathbf{J} = 4\pi\mathbf{u}/c$.

The law of induction for a fixed circuit is

$$V = c^{-1}\frac{\partial}{\partial t}\int(\mathbf{A}_1 + \mathbf{A}_2)\mathbf{ds} = c^{-1}\partial N/\partial t,$$

where $N = \int(\mathbf{BdS})$. This is equation (5.13).

3. Maxwell's Equations.

Previously we developed the aether theory of electromagnetics by generalising the scalar and vector potentials which emerged in formulating electrodynamic experiments. We can now proceed similarly when polarised and magnetised bodies are present. Consider the case of *constant* $\varkappa$ and μ, assumed to be interpreted statistically. We can take

$$\varphi = 1/\varkappa\,.\int\rho d\tau/r, \quad \mathbf{A} = \mu\int\mathbf{u}d\tau/cr$$

as formulae appropriate for stationary or quasi-stationary systems. The latter integral can be obtained from (10.15) by putting $\mathbf{w} = \mathbf{u}$ and curl $\mathbf{I} = (\mu - 1)/4\pi$, curl $\mathbf{H} = (\mu - 1)\mathbf{u}/c$. We can then generalise these as follows :

$$\varphi = 1/\varkappa\,.\int\rho d\tau/r]_{t-r/c'}, \quad \mathbf{A} = \mu\int\mathbf{u}d\tau/cr]_{t-r/c'}, \qquad (10.28)$$

where $c' = c/\sqrt{\varkappa\mu}$. Exactly as for (6.2) we deduce

$$\text{div}\,\mathbf{A} + \varkappa\mu\dot{\varphi}/c = 0.$$

From this and the equations

$$\mathbf{E} = -\nabla\varphi - \dot{\mathbf{A}}/c, \quad \mathbf{B} = \text{curl}\,\mathbf{A},$$

we obtain Maxwell's equations

$$c\,\text{curl}\,\mathbf{H} = 4\pi\mathbf{u} + \dot{\mathbf{D}},$$
$$c\,\text{curl}\,\mathbf{E} = -\dot{\mathbf{B}},$$

with $\mathbf{B} = \mu\mathbf{H}$, $\mathbf{D} = \varkappa\mathbf{E}$. This is the procedure usually adopted (e.g. Jeans, p. 569) ; it is equivalent to putting $\lambda = \varkappa\mu$ in (5.35) and (5.36).

But we have thereby ignored the expression $\mathbf{w} = \mathbf{u} + \dot{\mathbf{P}}$ for the current ; we took the current to be simply $\mathbf{u}$, the effect of matter being represented by the factors $\varkappa$ and μ. We have thus secured a correct phenomenological account by ignoring the statistical

analysis. But if we wish to take into account the proper interpretation of inductivity and permeability, we must generalise

$$\varphi = \int \rho d\tau/r \text{ and } \mathbf{A} = \int \mathbf{u} d\tau/cr$$

into

$$\varphi = \int (\rho + \rho')d\tau/r]_{t-r/c}$$

and

$$\mathbf{A} = \int \mathbf{w} d\tau/cr]_{t-r/c},$$

where $\mathbf{w} = \mathbf{u} + \dot{\mathbf{P}} + c \text{ curl } \mathbf{I}$. (10.29)

Assuming the equation of continuity

$$\text{div } (\mathbf{u} + \dot{\mathbf{P}}) + \partial/\partial t \, . \, (\rho + \rho') = 0,$$

this gives us

$$\text{div } \mathbf{A} + \dot{\varphi}/c = 0. \qquad (10.30)$$

Also

$$\nabla^2\varphi - \ddot{\varphi}/c^2 = - 4\pi(\rho + \rho'),$$
$$\nabla^2\mathbf{A} - \ddot{\mathbf{A}}/c^2 = - 4\pi\mathbf{w}/c. \qquad)10.31)$$

Hence, since $\dot{\mathbf{E}} = - \nabla\dot{\varphi} - \ddot{\mathbf{A}}/c$,

$$\text{curl } \mathbf{B} = \text{curl}^2 \mathbf{A} = - \nabla^2\mathbf{A} + \nabla \text{ div } \mathbf{A}$$
$$= 4\pi/c \, . \, (\mathbf{u} + \dot{\mathbf{P}} + c \text{ curl } \mathbf{I} + \dot{\mathbf{E}}/4\pi).$$

That is

$$c \text{ curl } \mathbf{H} = 4\pi\mathbf{u} + \dot{\mathbf{D}}.$$

Also

$$c \text{ curl } \mathbf{E} = - \text{ curl } \dot{\mathbf{A}} = - \dot{\mathbf{B}}.$$

And

$$\text{div } \mathbf{B} = 0,$$
$$\text{div } \mathbf{E} = 4\pi(\rho + \rho') \text{ so that div } \mathbf{D} = 4\pi\rho. \qquad (10.31\text{a})$$

We therefore obtain Maxwell's equations once more, without assuming $\varkappa$ and μ to be constant.

It will be observed from (10.31) and

$$\nabla^2\mathbf{B} - \ddot{\mathbf{B}}/c^2 = - 4\pi \text{ curl } \mathbf{w}/c,$$

that φ, $\mathbf{A}$ and $\mathbf{B}$ are now propagated with velocity c, not with $c/\sqrt{\varkappa\mu}$. And this is obviously the correct conclusion, the velocity c still holding even when $\varkappa$ and μ are not constant. For the whole point of the statistical considerations outlined in this section is that a material body is, as far as we are now concerned, not a material body but an aggregate of practically-point-charges in

vacuum. All effects are therefore propagated with the same velocity c as in the case of isolated electrons. The velocity $c/\sqrt{\varkappa\mu}$ is only an *apparent* velocity, resulting from complicated internal processes. It is clear therefore that the theoretically correct generalisation is contained in (10.29), and the answer to the problem raised in Chapter V is $\lambda = 1$ always. Notwithstanding, the value $\lambda = \varkappa\mu$ gives the correct phenomenological result for the case of constant $\varkappa$ and μ.

We can therefore write

$$\mathbf{B} = \mu \int \text{curl } \mathbf{u} \, . \, d\tau/cr]_{t-r/c'}$$

or

$$\mathbf{B} = \int \text{curl } \mathbf{w} \, . \, d\tau/cr]_{t-r/c}$$

or

$$\mathbf{B} = \int \text{curl } (\mathbf{w} + \dot{\mathbf{E}}/4\pi) \, . \, d\tau/cr.$$

The first expression gives us the apparent result when permeability and inductivity are constant and merely modify the result for vacuum in the way envisaged by Maxwell. The second gives us the theoretically correct result which in macroscopic practice is not apparent owing to complicated microscopic propagations. The third, which embodies Maxwell's so-called displacement-current, is merely a mathematically equivalent expression without physical significance. The integral supposes instantaneous propagation, and the term added to the true current is in reality a function of $\mathbf{A}$ and therefore of $\mathbf{B}$.

While therefore our statistical outlook has considerably modified our view of the process of propagation, it has not in any way upset the Poisson macroscopic analysis of polarisation. We can express $\mathbf{A}$ as a non-retarded integral from (10.29) and (10.31) :

$$\begin{aligned}\mathbf{A} &= \int \mathbf{u}_m d\tau/cr + \int d\tau/cr \, . \, (\dot{\mathbf{E}} + \nabla\dot{\phi}/4\pi) \\ &= \mathbf{A}_1 + \mathbf{A}' + \mathbf{C},\end{aligned}$$

where

$$\mathbf{A}_1 = \int \mathbf{w} d\tau/cr, \quad \mathbf{w} = \mathbf{u} + \dot{\mathbf{P}},$$

$$\mathbf{A}' = \text{curl} \int d\tau \mathbf{I}/r,$$

$$\mathbf{C} = \int d\tau/cr \, . \, (\dot{\mathbf{E}} + \nabla\dot{\phi}/4\pi).$$

The last integral **C** is due to the assumption of a finite velocity of propagation, its addition is a mathematical substitute for the retarded potential. The first ($\mathbf{A}_1$) is what we formerly called the electric vector potential; it may be obtained from (5.3) by putting $\lambda = 1$. The second ($\mathbf{A}'$) is the magnetic vector potential (5.6). We formerly had

$$\text{curl } \mathbf{A}' = -\nabla\varphi' + 4\pi\mathbf{I} \tag{5.9}$$

$$\mathbf{H} = -\nabla\varphi' + \text{curl } \mathbf{A}_1 \tag{5.11}$$

$$\mathbf{B} = \mathbf{H} + 4\pi\mathbf{I} = \text{curl } (\mathbf{A}_1 + \mathbf{A}') \tag{5.12}$$

We have now modified the treatment of Chapter V in two ways. (1) We have introduced propagation by means of the integral **C** instead of the second integral in (5.3), avoiding the hypothesis of $\varkappa_0$ not equal to unity. (2) We now regard all magnets, even permanent, as currents; so that $\nabla\varphi'$ is suppressed.

But observe that, in spite of our theoretical radicalism, we have not made any change in Maxwell's macroscopic equations. The first result of Ampère's hypothesis was to re-introduce **I** in (10.15). We still retain **B** and **H** in spite of the fact that in principle we are now bound to hold that we have nothing but moving point-charges and the forces between them, once we accept Ampère's assumption and the electron theory. We have not yet reached the necessary restatement of fundamental theory so long as we retain the auxiliary mathematical quantities which remain in our equations. As we have already pointed out, the substitution of **w** for **u** as current-intensity is essentially connected with purely practical limitations on our powers of observation. Any theory which is based on this distinction must lack ultimate physical significance.

To attain to a fundamental formulation we must revert to the electrons from which we started. We begin with the formulae

$$\mathbf{E} = -\nabla\varphi - c^{-1}\dot{\mathbf{A}},$$

$$\mathbf{F} = \mathbf{E} + c^{-1}V\mathbf{v} \text{ curl } \mathbf{A}.$$

As we shall presently see, these formulae, taken in their generality, by no means follow from the experiments on which they are supposed to be based. In fact they at once introduce the idea of absolute velocity which is not necessarily contained in the experimental results. At first we take

$$\varphi = e'/r, \quad \mathbf{A} = e'\mathbf{v}'/cr.$$

The next step is to generalise these into

$$\varphi = e'/R(1 - v'_R/c),$$
$$\mathbf{A} = \varphi \mathbf{v}'/c,$$

where R and $\mathbf{v}'$ refer to the time $t' = t - R/c$. This, as we have shown in (7.15), introduces only second-order terms in v'/c. Unfortunately it also introduces acceleration-terms, which are difficult, on the usual view, to reconcile with the permanent rotational or orbital motion of electrons.[6] Prescinding from this difficulty, we reach the next stage, which is the Liénard-Schwarzschild force-formula. This line of argument is far more fundamental than any manipulation with retarded potentials or with Maxwell's equations. The significant thing about the argument is its vulnerability; it proceeds not so much by logic as by happy intuitions.

Inasmuch therefore as Maxwell's equations for dielectric-magnetic bodies (1) are based on the electron theory, (2) introduce continuous integrals as a mere mathematical device, and (3) essentially involve the practical *de facto* limitations of measurement, we are not disposed to attach any great theoretical significance to their formulation. And, we may remark incidentally, this creates a presumption that Minkowski's application thereto of the theory of relativity, which claims to be ultimate and fundamental, may well be a purely mathematical expedient devoid of physical relevance.

We must next examine H. A. Lorentz's procedure for deriving Maxwell's equations for ponderable media, as it is nowadays reproduced in practically every text-book.[6a] He first writes down the equations 'for the free, i.e. the uncharged ether' (viii. 12):

$$c \operatorname{curl} \mathbf{E} = -\dot{\mathbf{H}},$$
$$\operatorname{div} \mathbf{H} = 0, \qquad (10.32a)$$

[6] That is, if we hold that a single electron radiates and do not adopt the view, advocated in Chapter VIII, that only statistical charge-aggregates initiate radiation.

[6a] Van Vleck says, p. 7: 'The formulation and proof of the statistical correlation of the macroscopic and microscopic equations is due originally to Lorentz.' What Lorentz invented was (10.32a) and (10.33), which Van Vleck calls (p. 2) the 'microscopic field equations.' Our point is that these micro-equations are a mathematical figment imposed on the electron-theory simply to enable us to use the integral calculus—for instance, to indulge in 'microscopic integration over one molecule' (Van Vleck, p. 11).

and

$$c \text{ curl } \mathbf{H} = \dot{\mathbf{E}},$$
$$\text{div } \mathbf{E} = 0. \tag{10.32b}$$

Lorentz waxes enthusiastic about these equations which, according to him, apply only to pure vacuum without charge or matter !

> The formulae for the ether constitute the part of electromagnetic theory that is most firmly established. Though perhaps the way in which they are deduced will be changed in future years, it is hardly conceivable that the equations themselves will have to be altered.—Lorentz, viii. 6.

Next, ' by the slightest modification imaginable ' (viii. 12) he assumes that when charges are present equations (10.32a) remain the same while equations (10.32b) become

$$c \text{ curl } \mathbf{H} = 4\pi\rho\mathbf{v} + \dot{\mathbf{E}},$$
$$\text{div } \mathbf{E} = 4\pi\rho. \tag{10.33}$$

The next step is described as follows :

> The equations for the ponderable media can be derived from the equations of the electron theory [i.e. 10.32a and 33], which hold microscopically in the smallest regions between and within the atoms and the electrons of which the ponderable bodies consist. . . . The method of deriving from them the equations for ponderable media consists in taking averages of the various magnitudes over regions which are large in comparison with the dimensions of the electrons and atoms, but small when compared with the dimensions of the bodies with which we are experimenting [i.e. over meso-domains].—Lorentz, xiii. 289 f.

We must first prove that, in the usual parlance, the operations of differentiating and averaging are commutative. Denoting the average or mean value by the suffix m, we have in general

$$\psi_m(x\, y\, z\, t) = \frac{1}{\tau t_m}\int_0^{t_m} d\theta \int d\tau\, \psi(x + a, \; . \; . \; . \; t + \theta), \tag{10.34}$$

where $d\tau = da\; db\; dc$ and the volume integral is taken within the limits $a^2 + b^2 + c^2 \leq l^2$. Hence, for fixed values of l and t_m,

$$\frac{\partial\psi_m}{\partial x} = \frac{1}{\tau t_m}\int d\theta \int d\tau \frac{\partial\psi}{\partial x} = \left(\frac{\partial\psi}{\partial x}\right)_m, \quad \frac{\partial\psi_m}{\partial t} = \left(\frac{\partial\psi}{\partial t}\right)_m.$$

Applied to Maxwell's equations, this gives (curl $\mathbf{E})_m$ = curl $\mathbf{E}_m$, etc. Hence we have :

$$\begin{aligned} c \operatorname{curl} \mathbf{E}_m &= -\dot{\mathbf{H}}_m \\ \operatorname{div} \mathbf{H}_m &= 0 \\ c \operatorname{curl} \mathbf{H}_m &= 4\pi(\rho\mathbf{v})_m + \dot{\mathbf{E}}_m \\ \operatorname{div} \mathbf{E}_m &= 4\pi\rho_m. \end{aligned} \tag{10.35}$$

These become identical with Maxwell's equations (10.31a), provided we take

$$\begin{aligned} \rho_m &= \rho + \rho', \\ (\rho\mathbf{v})_m &= \mathbf{u}_m = \mathbf{u} + \dot{\mathbf{P}} + c \operatorname{curl} \mathbf{I}, \end{aligned} \tag{10.36a}$$

and

$$\begin{aligned} \mathbf{E}_m &= \mathbf{E}, \\ \mathbf{H}_m &= \mathbf{B}. \end{aligned} \tag{10.36b}$$

Before proceeding to criticise this alleged proof, we observe that this last equation ($\mathbf{H}_m = \mathbf{B}$) has led to very curious conclusions. According to Mason-Weaver (p. 328), it 'indicates that **B** is the fundamental macroscopic vector.' In fact, according to another text-book (W. V. Houston, p. 221 f.), it shows that 'the average field inside a magnetised body is equal to the magnetic induction **B**.' The following quotations are to the same effect :

The symmetry of the electric and magnetic quantities is abandoned by the electron-theory.—Max Abraham, ii. 237.

The magnetic induction at any point is the mean magnetic force in a small region of space surrounding the point, **H** from the impressed field and $4\pi\mathbf{I}$ from the atomic orbits. The analogy with dielectrics fails here, for the mean electric force near a point in a dielectric is **E** and not **D**.—Pidduck, p. 226.

The total magnetic field, averaged for the small volume, is called the magnetic induction.—H. A. Wilson, *Enc. Brit.* 8 (1929[14]), 217.

The electric and magnetic cases are not entirely parallel. . . . **B** rather than **H** is the fundamental field vector.—Van Vleck, p. 3.

In the usual form of electron theory there is only one type of magnetic vector, say **h**. . . . It is clear without any detailed consideration that it is the macroscopic **B** (not **H**) which corresponds to the value of **h** averaged over a physically small volume. **B** thus corresponds to the total magnetic field, while **H** is simply the non-local part of the field.—Stoner, ii. 837.

The vector **H** is a compound quantity, while only **B** has an immediate significance as the mean value of the microscopic field-strength **h**.—Becker, p. 243.

The new point of view necessitates a revision of our previous definition of magnetic induction. . . . From the point of view of

Ampère's theory of magnetism which is universally accepted to-day, **B** inside a magnetic substance is the average resultant magnetic intensity, and **H** inside a magnetic material is not the magnetic intensity actually existing at all, but a fictitious field equal to the true magnetic intensity due to all outside sources (such as currents in wires) plus the magnetic intensity that would be produced by the fictitious magnetic charge—div **I** per unit volume. Outside magnetic media, **B** and **H** are the same and either represents the true magnetic intensity.—Leigh Page, ii. 379, 384.

According to this view, which is based exclusively on Lorentz's procedure just outlined, inside a magnetised body $\mathbf{H}_m = \mathbf{H}$ if we assume actual magnetic doublets but $\mathbf{H}_m = \mathbf{B}$ if we assume intra-molecular currents. In the textbook of Page-Adams (p. 275) an attempt is made to bolster up this assertion by a proof, based on the idea that the contribution to the mean magnetic intensity of the fields inside the equivalent magnetic shells is $-4\pi\mathbf{I}$, whereas the contribution of the current-field in this space is vanishingly small. Apart from the question whether 'magnetic field' has any meaning at all in such a context,[7] it should be noted that the principle of equivalence applies only *outside* the shells or doublets.

Other writers, starting from the same Lorentzian premiss (10.36b), go still further. Frenkel (ii. 15 f.) speaks of the 'inverted character' of the accepted definitions of **H** and **B**, and declares that the usual 'method of designation originated in the period when the same physical reality was ascribed to the magnetic substances as to the electric charges.' Accordingly he takes $\mathbf{H} = \mu'\mathbf{B}$, as also does Livens,[8] who tells us (iv. 674) that 'free space is thus the most permeable paramagnetic substance and the ferromagnetic media are almost impermeable.'

These assertions, which are so entirely contrary to all our previous analysis and which culminate in a violent paradox, lead us to suspect this entire procedure of Lorentz. And indeed the first thing that strikes us about his alleged proof is that it has no reference whatever to the electron theory; it does not in any way assume the Ampère-Weber theory of magnetism. From beginning to end it is based on that continuous analysis which we have already shown to be a purely mathematical expedient which works only because of the imperfections of our senses and instruments.

[7] In any case the mean field should be $\mathbf{B} - 8\pi\mathbf{I}/3$ and not **B**.

[8] Also Sommerfeld, ii. 815 f., led thereto by 'dimensions.'

The definition $\rho_m = \int \rho d\tau/\tau$ or in general $\psi_m = \int \psi d\tau/\tau$, taken over a meso-volume, is called 'averaging.' But it should more properly be called by some such term as 'continuisation.' It seems in fact to be implied that this averaging is a physical process, that the ρ or the ψ is really existent and continuously distributed, but that experimentalists can discern only its statistical mean value. Whereas in reality the operation is purely mathematical and artificial; it is not the definition of ρ_m but of ρ; so far from averaging a pre-existent quantity ρ, it introduces for the first time a mathematical fiction called ρ. The quantity $\rho_m = q/\tau$ has been arrived at by a *previous* process of averaging. We shall call ψ_m the meso-value; ψ may be called the micro-value, i.e. the fictitious quantity whose continuous distribution inside a meso-domain is mathematically equivalent to the average or meso-quantity ψ_m. It would seem then that the very definition (10.34) is inverted. It is assumed that ψ_m is a quantity obtained by certain mathematical operations, which have a physical significance, from a prior-existing quantity ψ which is continuously distributed over a meso-domain surrounding the point-moment $(x\,y\,z\,t)$; and also that ψ_m is a continuous function of $(x\,y\,z\,t)$. Whereas, applying this to density, we see that ρ_m is obtained by adding up the discontinuous charges in the meso-domain and dividing by the volume (and if necessary by a meso-duration). In order to deal easily with such a series of quantities ρ_m each pertaining to a macro-domain, we then invent a continuous distribution ρ in which the discontinuities are smoothed out.

Thus the operation of 'averaging' as envisaged by Lorentz is a purely mathematical manipulation without any physical consequence.

> The equations of the field [10.32a and 33]—which referred to dimensions small compared with the dimensions of an electron—may now be averaged over an element of volume of the size usual in mathematical physics—i.e. containing many groups of electrons.—G. T. Walker, ii. 35.

The allusion to electrons here evokes the idea of discontinuities; whereas this author, following Lorentz, regards a continuous field as fundamental:

> We may suppose that there is no outer surface of discontinuity bounding an electron, but that there is gradual transition from the electron to the empty aether.

We start, not with a statistical aggregate of point charges, but with a strictly continuous distribution; not with $\Sigma e\mathbf{v}$ over a meso-volume, but with $\mathbf{u}$ at a point. For some unexplained reason we then integrate this and other quantities, not over the entire region, but over a physically small domain. We take $\mathbf{u}_m = \int \mathbf{u}\,d\tau/\tau$, where τ is some fixed volume. But how do we get an equation such as

$$\mathbf{u}_m = \mathbf{u} + \dot{\mathbf{P}} + c \operatorname{curl} \mathbf{I}\ ?$$

Not the smallest reason is assigned or assignable. Why is it asserted that

$$\int \mathbf{H}\,d\tau = (\mathbf{H} + 4\pi\mathbf{I})\tau\ ?$$

The question of the imperfection of our practical mensuration has nowhere entered the argument. The quantity $\mathbf{H}$ is assumed to be continuously distributed; starting with this datum we then give the merely mathematical definition of $\mathbf{H}_m$ as the volume-integral of $\mathbf{H}$ divided by the volume. And there we remain; for experimental physics cannot be generated from the integral calculus. The quantities $\mathbf{P}$ and $\mathbf{I}$ do not emerge Venus-wise from integration. In other words, Lorentz's procedure is a delusion; and the *post factum* proofs and assertions invented to rehabilitate it merely show that, once a method has become fashionable in physics, reasons will be found for justifying it.

4. A Moving Medium.

If the material medium is itself moving with velocity $\mathbf{v}$ at any point, we can regard the current-density as made up of the following parts:

(1) the current existing independently of the motion $\mathbf{u}$;

(2) the convection current $(\rho - \operatorname{div} \mathbf{P})\mathbf{v}$;

(3) the polarisation stream which, by taking the time-rate (1.35) of the moving integral $\int (\mathbf{P}\,d\mathbf{s})$, is

$$\dot{\mathbf{P}} + \operatorname{curl} V\mathbf{P}\mathbf{v} + \mathbf{v} \operatorname{div} \mathbf{P};$$

(4) the current due to magnetisation $c \operatorname{curl} \mathbf{I}$.

Adding these, we have for the total current intensity

$$\mathbf{w} = \mathbf{u} + \rho\mathbf{v} + \dot{\mathbf{P}} + c \operatorname{curl} \mathbf{I} + \operatorname{curl} V\mathbf{P}\mathbf{v}. \qquad (10.37)$$

Obviously, from the standpoint of the electron theory, this proof is extremely unsatisfactory. Let us therefore try to extend formula (10.12) to cover this case. All we need to do is to substitute $\mathbf{v} + \dot{\mathbf{s}}$ for $\dot{\mathbf{s}}$ (which we there called $\mathbf{v}$). There will be two new terms on the right-hand side.

(1) The first is $\Sigma e\mathbf{v} = (\Sigma e_1)\mathbf{v}$, which gives $\int \rho \mathbf{v} d\tau$.

(2) The second is $\Sigma e\mathbf{v}(\mathbf{rs})/r^3$. Since

$$\Sigma e\mathbf{v}(\mathbf{rs}) = - V\mathbf{r}V\mathbf{pv} + \mathbf{p}(\mathbf{rv}),$$

the first part will clearly give

$$\int d\tau \text{ curl } V\mathbf{Pv},$$

just as $- cV\mathbf{r}\mathbf{M}$ gives $\int d\tau c \text{ curl } \mathbf{I}$.

We thus arrive at the formula (10.37). But we have still to deal with the term

$$\mathbf{p}(\mathbf{rv})/cr^3$$

in the expression for the vector potential. I must confess that I see no possibility of showing that this is statistically zero. Until someone solves this difficulty, equation (10.37) must be regarded as inconsistent with the argument based on (10.12).[9]

But, instead of the argument leading to (10.15), we can use the simpler argument which gives (10.19). We shall have to reserve until later (Chapter XII) the treatment of a moving circuit. But we can at once find the effect due to the moving polarisation. Let AB and $A'B'$ represent two positions of a doublet at the respective times t and $t + \delta t$ (Fig. 48). The displacement is equivalent to the transfer of $+ e$ from B to B' and of $- e$ from A to A', this latter being the same as the transfer of $+ e$ from A' to A. Now the lengths $AB = A'B' = a$ are negligible relatively to $AA' = BB' = v\delta t$, especially as δt must really be a meso-duration. Hence we can say that the transfer is equivalent to the transit of $+ e$ in a clockwise direction round the circuit $BB'A'AB$, the mean current being $j = e/\delta t$. Hence

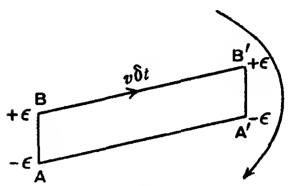

Fig. 48.

[9] The solution adopted in the argument leading to (10.19) practically amounts to taking $\mathbf{p} = 0$. That is, the magnetising electrons are considered separately and are, with the positive nuclei, regarded as forming a system of neutral currents.

by (4.8a) the transfer is equivalent to the creation of a magnetic doublet whose moment is

$$eV\mathbf{a}\mathbf{v}\delta t/c\delta t = Ve\mathbf{a}\mathbf{v}/c,$$

the vector **a** being AB, i.e. $e\mathbf{a}$ is the contribution of this particular doublet to the polarisation $\mathbf{p} = \Sigma e\mathbf{s}$. That is, for a meso-volume, the motion of the doublets is equivalent to the creation of a fictitious magnetic moment $V\mathbf{p}\mathbf{v}/c$. Or, using integrals, we can say that the effect of the motion is the same as if there were added a magnetisation-intensity

$$\mathbf{J} = V\mathbf{P}\mathbf{v}/c. \qquad (10.38)$$

Which of course may also be expressed as equivalent to a current-intensity

$$c \operatorname{curl} \mathbf{J} = \operatorname{curl} V\mathbf{P}\mathbf{v}. \qquad (10.39)$$

Therefore independently of any hypotheses which have led to formula (10.37), and apart from any dispute about an additional term, we have already proved the term c curl **I** and we have now proved the term curl $V\mathbf{P}\mathbf{v}$.

We could indeed prove (10.39) even more simply in the case of uniform polarisation. For by (1.6)

$$\operatorname{curl} V\mathbf{P}\mathbf{v} = -\mathbf{v} \operatorname{div} \mathbf{P} + (\mathbf{v}\nabla)\mathbf{P} - (\mathbf{P}\nabla)\mathbf{v} + \mathbf{P} \operatorname{div} \mathbf{v}.$$

We can regard v as constant throughout a meso-volume. So it is easy to see that this gives a surface-current-density $-\mathbf{v}$ divs $\mathbf{P} = \sigma'\mathbf{v}$, which we might have written down at once.

We have not, of course, assumed that the motion is linear, so we can now apply this result to experiments on rotating dielectrics.[10] Fig. 49 represents a hollow cylindrical disc of dielectric, of depth a and thickness b, which can be rotated round its axis. Above and below are metal rings, each with a small gap, which can be kept stationary or rotated. The

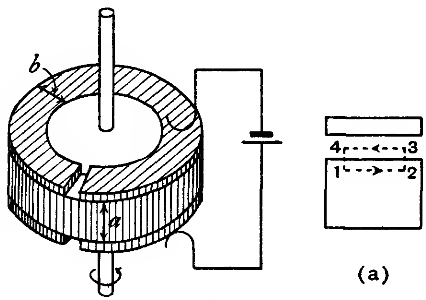

Fig. 49.

[10] O. W. Röntgen, AP 35 (1888) 264, 40 (1890) 93. Eichenwald, AP 11 (1903) 421, 872 and 13 (1904) 919. H. Pender, PR 15 (1902) 300. J. B. Whitehead, PZ 6 (1905) 474.

rings are connected to a battery so that the top ring is at a potential $+V$ elsts above that of the lower. Accordingly the electric intensity in the dielectric is $E = V/a$ downwards, and the polarisation is $P = (\varkappa - 1)V/4\pi a$ downwards. Hence on the upper metal ring surface density is $\sigma = \varkappa V/4\pi a$ and the density on the upper surface of the dielectric is $\sigma' = -P$. If both the rings and the dielectric are set in rotation, there is in the upper ring a convection stream

$$vb(\sigma + \sigma') = vbV/4\pi a.$$

If the rings are kept at rest, the current is

$$vb\sigma' = -vb(\varkappa - 1)V/4\pi a.$$

These results were confirmed by Eichenwald.

The vector $V\mathbf{Pv}$ is Pv radially outwards. Consider an infinitesimal rectangle 1234 as in Fig. 49a, with 12 of unit length just inside the dielectric and 34 in the air. Applying Stokes's theorem

$$\oint (V\mathbf{Pv}\ \mathbf{ds}) = \int (\text{curl } V\mathbf{Pv}.\ \mathbf{dS}),$$

the left-hand side is simply Pv, since P is zero for the side 34; and the right-hand is minus the current per unit width *down* through the paper. Thus the current-density curl $V\mathbf{Pv}$ gives a current $-bPv$, as we have just obtained more simply. We can also see that our result is equivalent to a magnetic intensity $J = V\mathbf{Pv}/c$, i.e. Pv/c radially outwards. It is easy to verify that this radial magnetisation would produce the same field as the convection-stream $-bPv$ elst. More simply still, the polarisation being uniform, we know that the current-densities are $v(\sigma + \sigma')$ and $v\sigma'$ respectively.

There have been many misinterpretations of these simple experiments. For instance:

> The experiments of Eichenwald confirm the immobility of the electromagnetic aether.—L. Bloch, p. 459.

On the contrary, in this as in other electromagnetic experiments when we employ Liénard's force-formula we always assume that the velocities are referred to the laboratory. Again we are told that

> the magnetic effects of displacement-currents can now be regarded as experimentally proved by the experiments of Eichenwald and Whitehead.—Graetz, p. 835.

Whereas obviously the experiments have nothing whatever to do with alleged displacement-currents. More serious, however, is the delusion that the results have some connection with Einstein's 'relativity.' It will here be sufficient to remark that the experiments are concerned not with the observations of two observers in uniform linear relative motion, but with the observations of one scientific observer concerning a dielectric rotating in the laboratory. This objection can even be discerned by reading the exposition of relativists themselves. Witness this quotation referring to Eichenwald's experiments :

> The rotary motion in the experiment is only a convenient means of communicating a sufficiently large measurable velocity to the dielectric. The result is usually taken as being true for any state of motion. Here we shall only consider the case of uniform rectilinear motion as a whole.—E. Cunningham, p. 132.

Prescinding from the alleged duality of observers, we may remark on the curious nature of the standpoint here disclosed. Formulae (10.38 and 39) can be proved by elementary reasoning for any kind of motion ; but it requires the highly metaphysical theory of relativity to prove the formulae for uniform linear motion. The contention sounds rather incredible.

5. Dispersion.

Let us examine the Maxwellian treatment of waves in a semi-conductor ($\varkappa$, μ, conductivity σ). Since the current is $\sigma\mathbf{E}$, we have

$$c \operatorname{curl} \mathbf{H} = 4\pi\sigma\mathbf{E} + \varkappa\dot{\mathbf{E}},$$
$$c \operatorname{curl} \mathbf{E} = -\mu\dot{\mathbf{H}}.$$

Also, $\varkappa$ and μ being constant, div $\mathbf{E}$ = div $\mathbf{H}$ = 0. Hence

$$\begin{aligned} -\nabla^2\mathbf{E} &= -\nabla \operatorname{div} \mathbf{E} + \nabla^2\mathbf{E} = -\operatorname{curl}^2 \mathbf{E} = c^{-1} \operatorname{curl} \dot{\mathbf{H}} \\ &= 4\pi\sigma c^{-2}\dot{\mathbf{E}} + \varkappa c^{-2}\ddot{\mathbf{E}}. \end{aligned} \tag{10.40}$$

We obtain a similar 'telegraphists' equation' for $\mathbf{H}$. If $\mathbf{E}$ contains the time-factor ε^{ipt}, the equation becomes

$$\nabla^2\mathbf{E} - \varkappa'\mu c^{-2}\ddot{\mathbf{E}} = 0, \tag{10.41}$$

where $\varkappa'$ is the complex quantity

$$\varkappa' = \varkappa - 4\pi i\sigma/p. \tag{10.42}$$

Suppose we are dealing with the plane-polarised wave

$$E = A\varepsilon^{ip(t - z/u)},$$

where $\varkappa'\mu = c^2/u^2$ so that u is complex. This is a solution of (10.41). Put

$$1/u = 1/v - i\alpha/c.$$

Then

$$\begin{aligned} \varkappa\mu - 4\pi i\sigma\mu/p &= \varkappa'\mu \\ &= c^2(1/v^2 - \alpha^2/c^2 - 2i\alpha/cv) \\ &= n^2 - \alpha^2 - 2in\alpha, \end{aligned}$$

where n is c/v. Hence

$$\begin{aligned} n^2 - \alpha^2 &= \varkappa\mu \\ n\alpha &= 2\pi\sigma\mu/p = \sigma\mu T, \end{aligned} \tag{10.43}$$

where $T = 2\pi/p = \lambda_0/c$ is the period, λ_0 being the wave-length in vacuum. The wave becomes

$$E = A\varepsilon^{-2\pi\alpha z/\lambda_0}\varepsilon^{ip(t - z/v)}$$

That is, the velocity is $v = c/n$, and α is the coefficient of extinction or absorption.

It is known that for optics we may take $\mu = 1$; the magnetising electrons begin to show effects only for $\lambda > 3$ cm. For water: $\mu = 1$, $\sigma = 7 \times 10^6$, $\varkappa = 80$, and

$$q \equiv 2\sigma T/\varkappa = 2\sigma\lambda/c\varkappa = 6 \times 10^{-6}\lambda$$

is small for light-waves (or even infrared $\lambda = 10^{-4}$). From (10.43)

$$\alpha/n = [(1 + q^2)^{\frac{1}{2}} - 1]q^{-1} \rightarrow \tfrac{1}{2}q.$$

Hence α should be negligible. But experiment shows that absorption is very great in the ultraviolet and increases with the frequency.

For metals $n^2 - \alpha^2 = \varkappa$ is untrue, since α is greater than n; also $n\alpha = \sigma T$ is incorrect at least if σ is taken as the static conductivity.

Alluding to the fact that $\varkappa$ is found not to be equal to n^2, Maxwell remarks (ii. 437): 'Our theories of the structure of bodies must be much improved before we can deduce their optical from their electrical properties.' The above-noted discrepancies show the failure of Maxwell's theory.

Let us therefore introduce electrons or point-charges, each being regarded as a harmonic oscillator with its own natural

frequency.[11] We are not interested in the modern complicated refinements, but only in the general theoretical change resulting from the introduction of these discontinuities. As we are concerned only with the statistical effect due to large groups of oscillators, we can use average values. If x is the displacement of an oscillator from its equilibrium position,

$$\ddot{x} + h\dot{x} + \omega_0^2 x = e/m \,.\, F. \tag{10.44}$$

We introduce a damping term ($h\dot{x}$) without having any clear views as to its causation. We can take the polarisation as $P = Nex$, where N is the number of oscillators per unit volume. The force F we take to be given by (10.10)

$$F = E + 4\pi/3 \,.\, Nex,$$

so that the equilibrium equation (10.10a) becomes

$$- m\omega_0^2 x_0 = - E - 4\pi/3 \,.\, Nex_0.$$

Hence (10.44) becomes

$$\ddot{x} + h\dot{x} + \omega^2 x = e/m \,.\, E, \tag{10.45}$$

where [12]

$$\omega^2 = \omega_0^2 - 4\pi Ne^2/3m.$$

Let us take $x = a\varepsilon^{ipt}$, i.e. the particular solution representing the forced vibration (of frequency $p/2\pi$) after the transients have died down. Then from (10.45) :

$$x = eE/m \,.\, (\omega^2 - p^2 + ihp).$$

Introduce the complex dielectric constant

$$\begin{aligned} \varkappa' &= 1 + 4\pi P/E = 1 + 4\pi Nex/E \\ &= 1 + 4\pi Ne^2/m\,(\omega^2 - p^2 + ihp). \end{aligned} \tag{10.46}$$

We must of course introduce the sign of summation before the last term if there are several different kinds of vibrating electrons.

Consider the plane-polarised disturbance travelling along z

$$E = A\varepsilon^{ip(t - qz/c)}$$

[11] 'This naïve depicture of an atom or molecule as a collection of harmonic oscillators is not in agreement with modern views of atomic structure as exemplified in the Rutherford atom, but yields surprisingly fruitful results.'—Van Vleck, p. 30.

[12] The formula $\mathbf{E} + c^{-1}V\mathbf{vH}$ 'can be limited to the first term in these problems. We suppose that the only magnetic force is that which comes from the light or heat vibrations. In these circumstances $\mathbf{H}$ is proportional to the vibration-velocities, and the last term is of the second order in these velocities. It can be neglected so long as the vibration-velocities are very small compared with the velocity of light.'—Lorentz, xviii. 103.

satisfying

$$\varkappa' c^{-2} \partial^2 E/\partial t^2 = \partial^2 E/\partial z^2$$

in formal agreement with Maxwell. We have

$$\varkappa'^{\frac{1}{2}} = q \equiv n - i\alpha,$$
$$\varkappa' = n^2 - \alpha^2 - 2in\alpha$$
$$E = A\ \varepsilon^{(2\pi\alpha z/\lambda_0)}\ \varepsilon^{[ip(t - nz/c)]}.$$

Equation (10.46) is of the form

$$\varkappa' = 1 + \frac{d}{a + ib} = 1 + \frac{ad}{a^2 + b^2} - i\frac{bd}{a^2 + b^2}.$$

Hence

$$n^2 - \alpha^2 - 1 = ad/(a^2 + b^2),$$
$$2n\alpha = bd/(a^2 + b^2),$$

where

$$a = \omega^2 - p^2, \quad b = ph, \quad d = 4\pi Ne^2/m.$$

That is,

$$n^2 - \alpha^2 = 1 + \frac{4\pi Ne^2/m\ .\ (\omega^2 - p^2)}{(\omega^2 - p^2)^2 + h^2p^2}$$

$$n\alpha = \frac{2\pi hpNe^2/m}{(\omega^2 - p^2)^2 + h^2p^2}. \tag{10.47}$$

We also have

$$(n^2 - \alpha^2 - 1)^2 + 4n^2\alpha^2 = d^2/(a^2 + b^2),$$

so that

$$2n\alpha/[(n^2 - \alpha^2 - 1)^2 + 4n^2\alpha^2] = b/d$$
$$= phm/4\pi Ne^2. \tag{10.47a}$$

Formulae (10.47) replace Maxwell's (10.43). Assume that there are only free electrons in metals. This is true in the infrared ($\lambda > 2$ microns). But in the visible spectrum and in the ultraviolet the optical behaviour of metals (except mercury) cannot be explained without also assuming bound electrons. Since $\omega = \omega_0 = 0$, (10.44) becomes

$$Ee/m = \ddot{x} + h\dot{x}.$$

Equation (10.47) gives

$$n^2 - \alpha^2 = 1 - 4\pi Ne^2/m\ .\ /(p^2 + h^2)$$
$$n\alpha = 2\pi hNe^2/mp(p^2 + h^2)$$
$$= \sigma' T,$$

where

$$\sigma' = Nhe^2/m[h^2 + (2\pi/T)^2]. \tag{10.49}$$

When $T = \infty$ we revert to the ordinary static conductivity

$$\sigma = \sigma'_{\infty} = Ne^2/hm.$$

For $\lambda > 25$ microns we have very approximately

$$n^2 = \alpha^2 = \sigma T.$$

Putting $p = 2\pi c/\lambda_0$, $\lambda' = 2\pi c/h$, $C = Ne^2/\pi mc^2$, we find

$$n^2 - \alpha^2 = 1 - C\lambda_0^2\lambda'^2/(\lambda_0^2 + \lambda'^2)$$
$$2n\alpha = C\lambda'\lambda_0^3/(\lambda_0^2 + \lambda'^2).$$

We have now removed the discrepancy with experiment, which characterised Maxwell's theory.

For gases and vapours $\omega = \omega_0$; and provided p is not very near ω, we can neglect $hp/(\omega^2 - p^2)$. In this case p^2/ω^2 is either small (for electrons whose natural frequencies are in the ultraviolet) or large (for those in the infrared). Hence according to (10.47)

$$n^2 = 1 + 4\pi\Sigma Ne^2/m(\omega_1^2 - p^2) - 4\pi\Sigma Ne^2/m(p^2 - \omega_2^2)$$
$$= 1 + A_1(p/\omega_1)^2 + A_2(\omega_2/p)^2 + \ldots$$

This is Ketteler's dispersion formula, which is found to agree with experiment. Abnormal dispersion (when p is nearly equal to ω) can similarly be explained.

The normal dispersion of fluids and solids is also given by (10.47). Neglecting $hp/(\omega^2 - p^2)$ we have

$$n^2 = 1 + \Sigma 4\pi Ne^2/m(\omega^2 - p^2)$$
$$= 1 + \Sigma A\lambda_0^2/(\lambda_0^2 - \lambda'^2)$$

This Ketteler-Helmholtz formula is found to agree with experiment. Without entering into further details, it is now clear that Maxwell's theory must be replaced by the electron theory, and that the Helmholtz-Duhem restatement is untenable.

As already pointed out, the damping coefficient h is merely assumed on general principles without any particular theory of its mechanism. The force $2e^2\dot{f}/3c^3$, already investigated (8.11a), is much too small to account for the observed absorption.[13] This force applies to scattering, not to the main part of the absorption process which, according to the quantum theory, is associated with the expulsion of electrons. In the case of scattering, since

$$\dot{x} = ipx,\ \ddot{x} = -p^2x,\ \dddot{x} = -p^3ix = -p^2\dot{x},$$

[13] Lorentz, viii. 141; Richardson, p. 267.

we have

$$mh\dot{x} = -2e^2/3c^3 \,.\, \ddot{x}$$

and accordingly

$$h = gp^2, \text{ where } g = 2e^2/3mc^3.$$

Substituting this value of h in (10.47), substituting Rayleigh's coefficient of transmission ($I = I_0\varepsilon^{-\beta z}$)

$$\beta = 4\pi\alpha/\lambda = 4\pi n\alpha/\lambda_0,$$

and remembering $p = 2\pi c/\lambda_0$, we obtain

$$\beta = \frac{4\pi gNe^2/m}{(\omega^2/p^2 - 1)^2 + g^2p}.$$

Or very approximately

$$\beta = 8\pi Ne^4/3m^2c^4(\omega^2/p^2 - 1)^2. \qquad (10.50)$$

This equation might also be obtained as follows. Suppose a plane wave $E = A\varepsilon^{ipt}$ falls on a doublet. Neglecting damping,

$$m(\ddot{x} + \omega^2 x) = eE$$

and the moment is

$$M = ex = e^2A/m(\omega^2 - p^2) \,.\, \varepsilon^{ipt}$$
$$= a\varepsilon^{ipt}.$$

From (8.71) the average rate of energy-emission is

$$U = p^4a^2/3c^3.$$

The scattering (β) is measured by the amount of light (NU) scattered per unit volume of material (N doublets scattering independently) divided by the intensity of the incident light, i.e. by the mean value of $cV\mathbf{EH}/4\pi$ which is $cA^2/8\pi$. This at once gives (10.50).

We can distinguish three cases :

(1) The fraction ω/p is large. This applies to light in the visible spectrum (p) and the natural frequency (ω) in the ultraviolet as for ordinary atoms. Neglecting 1 in the denominator of (10.50), we have

$$\beta = 8\pi Ne^4p^4/3m^2c^4\omega^4. \qquad (10.51)$$

The coefficient therefore varies as λ^{-4}, as is verified by experiment (blue light is scattered more than red). This is Rayleigh's formula,[14] which we shall express presently in a slightly different form.

[14] Rayleigh, PM 47 (1899) 379 ; *Scientific Papers*, iv. 397.

(2) The fraction ω/p is small, e.g. the incident radiation is X-rays. Neglecting ω^2/p^2 in (10.50), we have

$$\beta = 8\pi Ne^4/3m^2c^4,$$

so that the coefficient is independent of λ. This is J. J. Thomson's formula.

(3) Resonant scattering occurs when ω/p is nearly unity. In this case the denominator is very small; it is not zero, for we have neglected g^2p. Reinserting the factor, we obtain

$$\beta = 6\pi Nc^3/p.$$

This effect is much more pronounced than (1) or (2).

From (10.47a) we have approximately

$$2n\alpha(n^2-1)^{-2} = phm/4\pi Ne^2.$$

Or we can from (10.47) put

$$n^2 - 1 = 4\pi Ne^2/m(\omega^2 - p^2)$$

and replace the denominator in (10.50). Making the substitutions

$$p = 2\pi c/\lambda_0, \quad \beta = 4\pi n\alpha/\lambda_0, \quad n+1 \rightarrow 2,$$

we obtain

$$\beta = 32\pi^3(n-1)^2/3N\lambda_0^4 \qquad (10.52)$$

in Rayleigh's form.[15]

6. Magnetism.

We shall begin with a theorem due to Larmor (i. 341): For a monatomic molecule in a magnetic field, the motion of the electrons is approximately the same as the undisturbed motion (in the absence of a field) with a superposed common precession of frequency $\omega = He/2mc$. We neglect terms in H^2, we disregard the motion of the nucleus, and we assume e/m to be the same for all the point-charges. A charge $(-e)$, moving under a central force $\mathbf{R}$, a function of r, and a uniform magnetic field $\mathbf{H}$, has the equation of motion

$$m\mathbf{f} = \mathbf{R} - e/c \,.\, V\mathbf{vH}.$$

Referred to a system of axes rotating with ω about the centre of force, the velocity and acceleration become (1.7b, d)

$$\mathbf{v} = \dot{\mathbf{r}} + V\boldsymbol{\omega}\mathbf{r},$$
$$\mathbf{f} = \ddot{\mathbf{r}} + 2V\boldsymbol{\omega}\dot{\mathbf{r}},$$

[15] For experimental confirmation, see Dember, AP 49 (1916) 609.

where we have neglected $\dot{\omega}$ and ω^2 in the last equation. Hence approximately

$$m\ddot{\mathbf{r}} = \mathbf{R} + 2mV(\dot{\mathbf{r}}, \boldsymbol{\omega} - \mathbf{H}e/2mc).$$

Therefore if

$$\omega = He/2mc \tag{10.53}$$

the motion is the same as that of the original system only referred to rotating axes. We infer that the only influence of H is that the electrons, without change of internal motion, are set in rotation ω round the field. To justify our approximation take $e/mc = 1 \cdot 76 \times 10^{-7}$ (elm), n (the angular velocity of the electron radius-vector) $= 10^{14}$ sec.$^{-1}$, then

$$\omega/n = 0 \cdot 84 \times 10^{-7}\, H,$$

which is small since H at the greatest is of the order 10^5 gauss.

The change in the kinetic energy due to the precession is

$$\begin{aligned} \Delta W &= \tfrac{1}{2}m(v^2 - \dot{\mathbf{r}}^2) \\ &= m(\mathbf{v}V\boldsymbol{\omega}\mathbf{r}) \\ &= (\mathbf{p}\boldsymbol{\omega}), \end{aligned}$$

where $\mathbf{p} = mV\mathbf{r}\mathbf{v}$ is the angular momentum.

Since the magnetic field only exerts a force perpendicular to the electron's velocity, it cannot alter the energy. The change in the energy is due to the e.m.f. induced by the field as it increases from zero to H. Taking $\delta t =$ period T or $2\pi/n$, the e.m.f. round the orbit is

$$\begin{aligned} \oint (\mathbf{E}d\mathbf{s}) &= -\, c^{-1}\delta N/\delta t \\ &= -\, (\mathbf{S}\delta\mathbf{H})/Tc, \end{aligned}$$

where $\mathbf{S}$ the vector-area of the orbit is given by (10.16)

$$\mathbf{S} = \mathbf{p}T/2m.$$

Hence the energy supplied in one revolution is

$$\begin{aligned} \delta W &= -\, e\oint (\mathbf{E}d\mathbf{s}) \\ &= e/2mc\, .\, (\mathbf{p}\delta\mathbf{H}), \end{aligned}$$

and the whole increase in energy is

$$\Delta W = e/2mc\, .\, (\mathbf{p}\mathbf{H}) = (\mathbf{p}\boldsymbol{\omega}),$$

as already shown.

Suppose the electron is acted on by an 'elastic' force mn^2r towards the centre when disturbed from its equilibrium position. Then under the influence of a magnetic field

$$m\ddot{\mathbf{r}} = -\, e/c\, .\, V\mathbf{v}\mathbf{H} - mn^2\mathbf{r}.$$

Take H along z and we have

$$\ddot{x} + n^2 x = - eH/mc \,.\, \dot{y} = - 2\omega\dot{y},$$
$$\ddot{y} + n^2 y = eH/mc \,.\, \dot{x} = + 2\omega\dot{x},$$
$$\ddot{z} + n^2 z = 0.$$

Putting $x + iy = u$, we have

$$\ddot{u} - 2i\omega\dot{u} + n^2 u = 0,$$

the solution of which, ω/n being small, is

$$u = A\varepsilon^{i(n+\omega)t} + B\varepsilon^{i(-n+\omega)t}$$

Hence while the vibration along H is unaffected, the vibration frequencies in the xy plane become $n \mp \omega$, *i.e.* $\Delta n = \mp eH/2mc$. As is well known, this explains the normal Zeeman effect.[16] It was an important result as it gave the first estimate of e/m for the electron.

Larmor's theorem also provides a clue to diamagnetism. For electrons (charge $-e$) in quasistationary paths (10.14)

$$\mathbf{M}_0 = - e/2c \,.\, \Sigma V\mathbf{s}\mathbf{v}.$$

The effect of a magnetic field is to change the velocity to

$$\mathbf{v}' = \mathbf{v} + V\boldsymbol{\omega}\mathbf{s},$$

where $\boldsymbol{\omega} = \mathbf{H}e/2mc$. That is, the magnetic moment becomes

$$\mathbf{M} = \mathbf{M}_0 - e/2c \,.\, \Sigma V\mathbf{s}V\boldsymbol{\omega}\mathbf{s}$$
$$= \mathbf{M}_0 - e/2c \,.\, [\boldsymbol{\omega}s^2 - \mathbf{s}(\boldsymbol{\omega}\mathbf{s})].$$

Taking ω along z:

$$M_x = M_{0x} + e\omega/2c \,.\, \Sigma xz$$
$$= M_{0x},$$

since the average value of Σxz is zero. Also

$$M_z = M_{0z} - e\omega/2c \,.\, \Sigma(x^2 + y^2)$$
$$= M_{0z} - e^2H/6mc^2 \,.\, \Sigma s^2,$$

since, for a cubically symmetrical or for a random distribution, $\Sigma x^2 = \Sigma s^2/3$. If there is no permanent magnetism ($\mathbf{M}_0 = 0$), if there are N atoms per unit volume, Z electrons in an atom and a^2 the mean square distance from the centre ($\Sigma s^2 = Za^2$), then $I = \chi H$, where

$$\chi = - NZe^2a^2/6mc^2. \tag{10.54}$$

[16] Zeeman, PM 43 (1897) 226.

We can suppose that all substances are diamagnetic, but that this effect is overpowered in the case of certain substances. Langevin was the first to develop Weber's theory of magnetism. In 1905 he investigated the case when the magnetic susceptibility is due entirely to the orientation of permanently polarised molecules resisted by temperature agitation.[17]

Suppose there are N molecular doublets per unit volume. Each doublet has potential energy

$$\psi = -MH \cos \alpha,$$

where α is the angle between M and H. There is statistical equilibrium between the orientating effect of the field and the disturbing effect of thermal motion. According to Boltzmann's distribution-law, $nd\omega$ is the number of doublets per unit volume pointing in the direction α, contained in the solid angle $d\omega = \sin \alpha d\alpha d\varphi$, where

$$n = A\varepsilon^{-\psi/k\theta},$$

k being Boltzmann's gas-constant and θ the absolute temperature. Or approximately

$$n = A(1 + a \cos \alpha),$$

where $a = MH/k\theta$ is taken to be small. The number per unit volume is

$$\begin{aligned} N &= \int nd\omega \\ &= 2\pi A \int_0^\pi d\alpha \sin \alpha(1 + a \cos \alpha) \\ &= 4\pi A. \end{aligned}$$

Each doublet has a component $M \cos \alpha$ in the direction of H. Hence the total moment per unit volume in this direction is

$$\begin{aligned} I &= \int M \cos \alpha \, . \, nd\omega \\ &= 4\pi AMa/3 \\ &= NM^2/3k\theta \, . \, H. \end{aligned}$$

That is

$$\chi = NM^2/3k\theta. \tag{10.55}$$

[17] Debye applied the same theory to explain part of *electric* susceptibility.—PZ 13 (1912) 97. By introducing the theory of permanent electric doublets he was able to account for the temperature coefficient of the dielectric coefficient of alcohols. Modern developments of these ideas are beyond the scope of this book.

In 1895 Curie discovered the experimental law $\chi = C/\theta$ for paramagnetic solutions. Weiss's law $\chi = C/(\theta - \theta_0)$ was subsequently found to hold very well for the great majority of paramagnetic salts and for ferromagnetics above the Curie point. This is arrived at as follows. In the above proof H should really be replaced by $H + NI$, the total field, where according to our previous simple theory $N = 4\pi/3$. Hence we have

$$I/(H + NI) = C/\theta$$

or

$$\chi = C/(\theta - \theta_0),$$

where $\theta_0 = NC$. For nickel $N = 13700$ and for iron and cobalt the values are of the same order of magnitude. We conclude then that 'it is impossible that the molecular field should be produced by the elementary magnets according to the ordinary laws of magnetism.' [18]

It would seem then that we have taken the first step towards explaining paramagnetism, and perhaps even ferromagnetism. But subsequent investigation has had to condemn the effort. Langevin's theory assumes—without 'classical' justification—that all the molecules (of the same chemical composition) possess the same permanent invariable magnetic moment, and therefore that the circulating electrons have the same angular momentum.[19] It would seem that the molecular field phenomena both in para- and ferro-magnetics must be attributed, not to purely magnetic interaction of the carriers of the magnetic moment, but to complicated processes of electron-interchange between groups of atoms. We must therefore resign ourselves to the fact that magnetism, like other atomic properties, is not so simple as we thought and requires the quantum theory for its treatment.

Anticipating a notation and an argument which will be introduced in Chapter XIV, we can easily see that Langevin's formula (10.55), or Weiss's modification of it, must (apart from the factor 1/3), occur, no matter what theory we adopt. I is clearly proportional to N, and we assume that it depends only on H, the magnetic moment M, and the average energy of a molecule which

[18] P. Weiss et G. Foex, *Le magnétisme*, 1931², p. 168.

[19] Miss J. H. van Leeuwen, JP 2 (1921) 361 ; Van Vleck, p. 94 ; Stoner, i. 126. Classical statistical theory presupposes that the energy associated with any degree of freedom of an element is capable of continuous variation.

we take to be $3k\theta/2$ (strictly applicable only to a gas or a dilute solution). Then,[20] in accordance with (14.10),

$$I/NM = f(MH/k\theta) \rightarrow CMH/k\theta. \tag{10.56}$$

From 1914 to 1924 Barnett made interesting experiments on magnetisation by rotation. When a magnetised body is rotated the magnetic elements, if they have angular momentum, behave like gyrostats, i.e. they change their orientation so that the direction of rotation tends to coincide with the direction of the impressed rotation. But owing to torques due to adjacent elements, only a slight change of orientation can occur. It is found that the rotated body is magnetised as if the action of negative elements were preponderant.

> The investigation gave a direct proof—and the first proof—of the actual existence in iron of the molecular currents of Ampère, before hypothetical; it proved that the electricity in these currents is negative and has mass or inertia.—Barnett, xi. 253.

But the ratio R of formula (10.16a) instead of being $2mc/e$ was found to be $1 \cdot 05\ mc/e$ on the average for ferromagnetics. So Barnett concludes (xi. 245) that 'the magnetic element consists primarily of a Lorentz electron spinning on a diameter and not of electrons moving in an orbit.' To understand this let us consider an electrified spherical surface in slow uniform rotation about a diameter. At outside points the magnetic field H is that due to a doublet $M = \frac{1}{3}a^2e\omega/c$. We can calculate the angular momentum about the axis by assuming the mathematical fiction of spatially distributed electromagnetic momentum.[21] At a point distant r (at θ with the axis) from the centre, the density is

$$g = EH_\theta/4\pi c = eM \sin\theta\ .\ /4\pi r^5 c.$$

The angular momentum (moment of momentum) can be represented by an integral taken outside the sphere :

$$P = \int d\tau g r \sin\theta = 2eM/3ac.$$

But the mass is $m = 2e^2/3ac^2$ when e is in elsts. Therefore

$$R = P/M = mc/e.$$

Similarly for a solid sphere we find $R = 5mc/7e$.

[20] For $[MH] = W$ (measure-ratio of energy) $= [k\theta]$. Hence I/NM and $MH/k\theta$ are tautometric.

[21] *Cf.* M. Abraham, i. 171. This of course is based on the unsatisfactory idea of 'electromagnetic mass' which we criticised in Chapter VIII.

Before briefly commenting on this argument, let us glance at the converse experiment, rotation by magnetisation, initiated in 1915 by Einstein and de Haas. (References will be found in Barnett, xi. 258.) When a freely suspended rod is magnetised so that its magnetic moment is M, it will (since total angular momentum is conserved) acquire an angular momentum $-RM$. It is found that approximately $R = mc/e$; e.g. for iron the coefficient is $1 \cdot 037$. So once more we obtain only half the value given by formula (10.16a). The conclusion is that in ferromagnetics the effective factor is the spin moment of the electrons in the atoms and ions—not the free electrons which should show only a slight paramagnetic effect.

Apart from spectroscopy (e.g. the anomalous Zeeman effect) there is further evidence that the elementary magnet must be the electron or nucleus.[22] This result is disconcerting since it implies failure of the Weber-Amperian analysis previously given. Magnetic fields produced by ordinary currents are due to the drift-velocity of electrons, while the magnetic field of iron is due to elementary magnets hitherto regarded as point-charges. There is thus a surprising dichotomy between current-magnetic and ferro-magnetic fields.

Accordingly it has been advocated that the electron should be regarded as a ring or as a sphere of ' electricity.'

> The essential assumption of this theory is that the electron is itself magnetic, having in addition to its negative charge the properties of a current circuit whose radius—finally estimated to be $1 \cdot 5 \times 10^{-9}$ cm.—is less than that of the atom but of the same order of magnitude. Hence it will usually be spoken of as the magneton. It may be pictured by supposing that the unit negative charge is distributed continuously around a ring which rotates on its axis with a peripheral velocity of the order of that of light, and presumably the ring is exceedingly thin.—A. L. Parson, ' A Magneton Theory of the Structure of the Atom,' *Smithsonian Misc. Collections*, 1915, p. 3.
>
> Up to this point we have considered the electron merely as a point-charge revolving about a nucleus. To explain the doublet nature of the terms of the alkali spectra, it is necessary to introduce a new concept which is supported by considerable experimental evidence. We assume that the electron is spinning about an axis passing through its centre of gravity. . . . Since a spinning electron

[22] For example the X-ray diffraction pattern of magnetite, etc.—Compton and Trousdale, PR 5 (1915) 315 ; the transmission of X-rays through magnetised iron.—Forman, PR 7 (1916) 119.

of a finite volume is equivalent to a circular current, a magnetic field is associated with its spin and it acts as a tiny magnet. A magnetic field is associated also with the orbital motion of the electron.—*Outline of Atomic Physics*, by Members of the Physics Staff of the University of Pittsburgh, 1933, p. 159 f.

We here impinge upon the limits of 'classical' theories; all we can do in this book is to record the difficulty. In addition to ferromagnetism there are phenomena, not dealt with here, which lead to the theory that the electron is an extended entity with a definite spin round an axis—that it possesses angular momentum $P = h/4\pi$ and magnetic momentum $M = Pe/m$—in addition to its orbital motion. Unless we hold the theory of Boscovich, there is no difficulty in accepting an extended electron. The objections urged in Chapter VIII referred to a different issue. We know that for ordinary electromagnetic phenomena, which relate to statistical effects, electrons may be regarded as point charges obeying Liénard's—or, as we shall see, Ritz's—force-law. What is extremely doubtful is whether we are justified in taking this law, statistically applicable to electrons as wholes, as acting between sub-electronic elements within an electron. Lorentz's argument, based on the apparent variation of mass, we found to be unsatisfactory; and in the next chapter we shall suggest an alternative. Stricter relativists, as we have seen, regard the electron as a point-charge, not differing (as regards mass-variation) from an unelectrified particle; but this theory appears merely to be an algebraic manipulation. In the wave-mechanical theory of Dirac, the electron is similarly treated; the 'spin' phenomena result from the fact that the electron's motion is characterised by two (or four) wave-functions.

Whatever way we regard the results there is a serious difficulty. If, in order to unify our view of 'magnetic field,' we regard the electron as compacted of continuously distributed negative charge, rotating round an axis, we have to explain why such an unstable aggregate does not explode. If, on the other hand, we regard 'charge' as characterising the entity as a whole, we must then postulate a 'magnetic moment' independently; we cannot then explain H in the same way as we do in the case of ordinary currents or moving charges. The difficulty remains, it is not dissipated even by using tensors or matrices.[23]

[23] *Cf.* the attempt of L. H. Thomas (by special relativity).—PM 5 (1927) 1.

CHAPTER XI

WEBER—RITZ

1. Ritz.

We have seen that the derivation of the Liénard-Schwarzschild force-formula is neither direct nor intuitive; it proceeds from such theoretical constructs as scalar and vector potential-waves. Since the advent of the electron theory the electromagnetic theory of light has lost that apparently simple form it had in Maxwell's time when **E** and **H** could be regarded as indicating physical entities, states of the aether. Many alternative phraseological descriptions have been attempted. For example, Sir J. J. Thomson's ' view of light as due to the tremors in tightly stretched Faraday tubes,' which are ' discrete threads embedded in a continuous ether ' (xi. 62). And there have been many other more peculiar descriptions, which are accepted as perfectly legitimate. Prof. Bateman (ii. 143) expresses a view which, he thinks,' may throw light on the nature of the aether.' It is this:

> If the aether is supposed to be made up of electricity travelling along straight lines with velocity *c* [relative to what ?], the electromagnetic fields may be produced by collisions between the aether-particles. An aether particle may consist normally of an electric doublet with velocity *c*.

We may also quote the view adopted by Prof. Leigh Page:

> Electric charges are assumed to be the fundamental constituents of matter, magnetic poles existing only as secondary entities. Each element of electricity is supposed to emit uniformly in all directions with the velocity of light continuous streams of moving elements or, as Bateman has termed them, light particles. Each moving element travels out from its source in a straight line uninfluenced by the subsequent motion of the source or by the presence or motion of neighbouring moving elements. The nature of these moving elements is immaterial for the purposes of the theory other than that each must be susceptible of continuous identification and must move in a straight line with the velocity of light. A line of electric

force is defined as the locus of a stream of moving elements emerging from a single source. To employ an analogy, a source may be likened to a machine gun firing bullets with the velocity of light; the bullets correspond to moving elements. If they are supposed to be strung along an endless perfectly elastic thread, such a thread constitutes a line of force.—Page, v. 292.

We are forced to conclude that a line of force is to be considered as a locus of points, each of which is moving in a straight line with the velocity of light. These points will be named 'moving elements.' . . . It has no such properties as mass or energy associated with it. In fact the representation of the field by moving elements is purely kinematical in character, in no sense dynamical. . . . We picture a point-charge as a source of streams of moving elements shot out in all directions with the velocity of light. . . . Moving elements . . . cannot be deflected or slowed down by matter. . . . They perform the functions of the elastic ether of pre-relativity days.—Leigh Page,[1] x. 223 f., 231.

These statements are not cited for their intrinsic interest; though the last, if logically developed, has analogies with a new view to be presently explained. In fact these so-called alternative theories are mostly metaphorical roundabout descriptions of the argument which we have expressed in a more direct and straightforward way by the retarded potentials. The point is that no one is shocked at them, however far-fetched and outlandish they may be. For they all fall within the conventional limits of present-day orthodoxy in physics. They all have this in common, that they are based on absolute or non-relative velocities. They would indeed have shocked physicists in the Gauss-Weber epoch, but they excite no particular surprise in the Maxwell-Lorentz era. Fashions change in science as in millinery.

But if, greatly daring, one were to-day to revert to the views of Gauss, Weber and Riemann, if one professed to be *really* relativist and to eschew absolute velocities, then indeed one would have passed the contemporary allowable limits of scientific tolerance. 'If I had openly expressed such heterodox opinions,' says Sir A. Schuster (ii. 59), referring to the electron theory as viewed a generation ago, 'I should hardly have been considered a serious physicist, for the limits to allowable heterodoxy in science are soon reached.'

[1] This is in no sense an 'emission theory of electromagnetism,' for it is based upon medium-kinematics. As Prof. Page says (x. 224), 'the words "stationary" and "moving" always refer to the system of the observer,' i.e., in plain English, to the laboratory (convected aether). Moreover—though the matter will not be discussed here—he has not really proved his 'theory.' But it forms the Liénard analogue of Ritz's idea.

And when, in spite of his acknowledged researches in spectroscopy and elasticity, the Swiss physicist, Walther Ritz, expressed heterodox views on electromagnetics in 1908, shortly before his death, his ideas were received with a chill silence and have ever since been systematically boycotted. He was out of tune with the music, out of step with the crowd. He wrote in a letter in 1908 (p. xx):

> I am now going to return to the optics of bodies in motion, to satisfy my conscience but without enthusiasm. I cannot indeed doubt that people will approach my ideas, whatever be the perfection I give them, only with extreme misgiving; a conversation with X after many other conversations has convinced me of this. Nobody can give me a valid objection, and I have silenced X himself. But that makes no difference—they find my ideas monstrous (*scheusslich*).

Nevertheless, in spite of the X's, we intend to expound here the views expressed by Ritz, which represent the most important and interesting attempt to carry on the pre-Maxwellian tradition of an electron theory *without* an aether.

Ritz's initial assumption (p. 372) is that each electrified point emits, at each instant and in all directions, fictitious infinitely small particles, all animated with the same radial velocity c relative to the origin; so that the aggregate of the particles emitted at the instant t' by a moving electron S forms at any subsequent instant t a sphere of radius $\rho = c\,(t - t')$.

> The principle of relativity of motion in its classical form [he says, p. 443] requires that the waves emitted by a system in uniform motion, not subjected to any external sensible influence, should move with this system so that the centre of each spherical wave continues to coincide with the electron which has emitted it and the radial velocity is constant and equal to c. If the motion of the electron is variable, the principle of relativity no longer determines the velocity of displacement of the wave-centre; but this velocity must be constant, otherwise there would be action at a distance between the electron and the emitted wave.

Hence (Fig. 50) the centre of the wave or sphere emitted by S at time t' is situated at S_1 at the time t, where SS_1 is vectorially $\mathbf{v}'(t - t')$, i.e. the centre is at the point where S would be if it had continued to move with the velocity $\mathbf{v}'$ which it had at the instant of

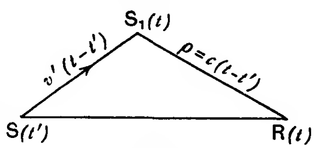

Fig. 50.

emission. It is understood that the system is referred to any set of Newtonian inertial axes. If $(x'y'z')$ are the coordinates of the source S at time t', and if (xyz) are those of the receiver R which the sphere reaches at time t, then

$$\rho_x = x(t) - x'(t') - (t - t')v_x'(t').$$

If

$$\mathbf{U} = \mathbf{v}(t) - \mathbf{v}'(t')$$

and $\mathbf{w}$ is the velocity of the emission relative to R,

$$\begin{aligned} w_x &= c \cos(\rho x) + v_x' - v_x \\ &= c \cos(\rho x) - U_x. \\ w_\rho &= \Sigma w_x \cos(\rho x) = c - U_\rho \\ w^2 &= c^2 - 2cU_\rho + U^2. \end{aligned} \tag{11.1}$$

In view of our previous discussions, there can be no valid objection to this assumption as a scientific hypothesis. It is not one whit more peculiar or incredible than the assumption of potential-waves, on which the rival theory rests. It is in fact the plain statement of the alternative kinematic scheme: a ballistic theory based on purely relative velocities *versus* a medium theory based on absolute velocities. Instead of waves in a medium we have emissions projected. 'The particles,' explains Ritz (p. 321), 'are simply the concrete representation of the kinematic and geometrical data.'

Suppose a sphere is emitted from e' at time t', then at time t the coordinates of its centre are $x'(t') + (t - t')v_x'(t'), \ldots$, and

$$\begin{aligned} c^2(t - t')^2 &= \Sigma \rho_x^2 \\ &= \Sigma[x - x' - (t - t')v_x']^2. \end{aligned}$$

At time $t' + dt'$ another sphere is emitted, and at time t its centre is at the point

$$x' + (t - t')v_x' + (t - t')dt'f_x' \ldots,$$

where $\mathbf{f}'$ is the acceleration of e' at the moment t'. If $(X + x, \ldots)$ are the coordinates of a point near $R(xyz)$ through which the second sphere passes at time t,

$$c^2(t - t' - dt')^2 = \Sigma[X + x - x' - (t - t')v_x' - (t - t')dt'f_x']^2.$$

Subtracting these two equations, we have

$$-c^2(t - t')dt' = \Sigma\rho_x\{X - (t - t')dt'f_x'\}.$$

That is

$$\Sigma\rho_x X = -c^2(t - t')dt'(1 - \Sigma f_x'\rho_x/c^2),$$

or

$$\Sigma X \cos(\rho x) = -cdt'(1 - \rho f_\rho'/c^2).$$

This is the equation of the tangent-plane to the sphere, and its perpendicular distance from the origin R is the normal distance between the two spheres :

$$dn = -\, c\,dt'(1 - \rho f'_\rho/c^2).$$

Or we might proceed thus.[1a] If the two spheres emitted at times t' and $t' + dt'$ are at a normal distance dn at the point (xyz) at time t,

$$\begin{aligned} dt' &= dn\Sigma \frac{\partial t'}{\partial x} \cos(\rho x) \\ &= dn\left(\frac{\partial t'}{\partial n}\right)_{t=\mathit{const.}} \\ &= -\frac{dn}{c}\frac{\partial \rho}{\partial n}, \end{aligned}$$

since $\rho = c(t - t')$. Now, since $t' = t - \rho/c$,

$$\frac{\partial t'}{\partial x} = -\frac{1}{c}\frac{\partial \rho}{\partial x}.$$

And, since $\rho_x = x - x' - v'_x\rho/c$,

$$\begin{aligned} \rho\frac{\partial \rho}{\partial x} &= \rho_x + \frac{\rho}{c^2}(\Sigma f'_x\rho_x)\frac{\partial \rho}{\partial x} \\ &= \rho \cos(\rho x) + \frac{\rho^2 f'_\rho}{c^2}\frac{\partial \rho}{\partial x}. \end{aligned}$$

Whence

$$\partial\rho/\partial x = \cos(\rho x)\,.\,/(1 - \rho f'_\rho/c^2),$$

so that

$$\partial\rho/\partial n = 1/(1 - \rho f'_\rho/c^2).$$

Hence, once more,

$$dt' = -\, dn/c(1 - \rho f'_\rho/c^2).$$

If we admit that the number of particles emitted in time dt' is proportional to $e'dt'$, the number of particles situated in an element dS of the sphere will be proportional to $e'dS/\rho^2$. Hence

[1a] Or, as on p. 213, taking $(X'Y'Z')$ as coordinates relative to axes moving with $S(t')$ with constant velocity $v'(t')$, we have

$$X + x = X' + x'(t') + (t - t')v'_x.$$

Since t is constant and $c(t - t') = \rho$, the Jacobian

$$\begin{aligned} \partial(X'Y'Z')/\partial(XYZ) &= 1 - (t - t')\Sigma f'_x \partial t'/\partial X' \\ &= 1/(1 - \rho f'_\rho c^{-2}). \end{aligned}$$

the number contained in the volume-element $dSdn$ between the two spheres is proportional to

$$\frac{dt'e'dS}{\rho^2} = -\frac{1}{c}\frac{e'}{\rho^2}\frac{\partial\rho}{\partial n}\,dSdn.$$

Therefore the density may be taken as

$$D = \frac{ae'}{\rho^2}\frac{\partial\rho}{\partial n} = \frac{ae'}{\rho^2(1-\rho f'_\rho/c^2)}, \qquad (11.2)$$

where a is a constant.

On the aether-electron theory, which for brevity we shall call the Lorentz theory,

$$t' = t - R/c, \quad \partial t'/\partial x = -1/c\,.\,\partial R/\partial x$$
$$R^2 = \Sigma(x-x')^2$$
$$R\frac{\partial R}{\partial x} = R_x\left(1 - \frac{\partial x'}{\partial t'}\frac{\partial t'}{\partial x}\right) - R_y\frac{\partial y'}{\partial t'}\frac{\partial t'}{\partial x} - R_z\frac{\partial z'}{\partial t'}\frac{\partial t'}{\partial x}$$
$$= R\cos(Rx) + R\frac{\partial R}{\partial x}\frac{v'_R}{c}$$

Hence

$$\partial R/\partial x = \cos(Rx)\,.\,/(1 - v'_R/c)$$
$$\partial R/\partial n = 1/(1 - v'_R/c).$$

Our next step is really a tentative proceeding by analogy with formula (7.14), which can be expressed as

$$F_x = \frac{ee'}{R^2}\frac{\partial R}{\partial n}\left[A'\cos(Rx) + B'\frac{v'_x}{c} + C'\frac{Rf'_x}{c^2}\right].$$

We assume that $\mathbf{F}$ depends only on the disposition and velocities of the neighbouring particles, i.e. on $\mathbf{w}$, ρ, D and the first derivatives of ρ (which introduce the acceleration). This is a natural assumption on an emission theory, as it is also to take the force to be proportional to e and D. That is,

$$F_x = eD[A_1\cos(\rho x) + B_1 w_x/c + C_1\rho f'_x/c^2],$$

where A_1, B_1, C_1 are independent of the coordinates, being functions of ρ, w^2, w_ρ. Hence from (11.1) and (11.2),

$$F_x = ee'/\rho^2(1-\rho f'_\rho/c^2)\,.\,[A\cos(\rho x) - BU_\rho U_x/c^2 - C\rho f'_x/c^2], \quad (11.3)$$

where A, B, C are functions of u^2/c^2 and U_ρ/c, which are independent of ρ; and we make the assumption, which is not indispensable, that they are quadratic functions of U_ρ/c.

Having tentatively obtained an emission-formula to replace the

medium-formula of Liénard, let us obtain an approximate form of it comparable with (7.17), for the case in which the expansions

$$x'(t-\rho/c) = x'(t) - \rho/c \,.\, v'_x(t) + \rho^2/2c^2 \,.\, f'_x(t) + \ldots$$
$$v'_x(t-\rho/c) = v'_x(t) - \rho/c \,.\, f'_x(t) + \ldots \qquad (11.3a)$$

are very convergent. Let us denote the *simultaneous* distance by r, so that $r_x = x - x'$, where we omit the argument t. Then approximately

$$\rho_x = x - x'(t-\rho/c) - \rho/c \,.\, v'_x\,(t-\rho/c)$$
$$= x - x' + \rho^2/2c^2 \,.\, f'_x.$$
$$\rho^2 = \Sigma\rho_x{}^2 = r^2 + r\rho^2/c^2 \,.\, f'_r. \qquad (11.3b)$$

Hence

$$\rho^2 = r^2/(1 - rf'_r/c^2) = r^2(1 + rf'_r/c^2 + \ldots)$$
$$\rho_x = r_x + r^2/2c^2 \,.\, f'_x$$
$$\cos(\rho x)/\rho^2 = \rho_x/\rho^3 = \cos(rx)/r^2 \,.\, (1 - 3rf'_r/2c^2) + f'_x/2c^2r.$$

Also (11.3c)

$$U_\rho = \Sigma[v_x - v'_x(t-\rho/c)]\rho_x/\rho$$
$$= \Sigma(v_x - v'_x)\cos(rx) + rf'_r/c$$
$$= v_r - v'_r + rf'_r/c.$$

Since U^2 and U^2_ρ occur in F only with the factor $1/c^2$, they need not be developed beyond the first term, so that we can take U^2 to be the actual relative velocity (u^2):

$$U^2 = u^2 = \Sigma(v_x - v'_x)^2,$$
$$U^2_\rho = u^2_r = (dr/dt)^2.$$

Substituting these approximations in (11.3), we obtain

$$F_x/ee' = \{1 + rf'_r/c^2\}/r^2 \,.\, [\cos(rx)\{(1 - 3rf'_r/2c^2)\}A$$
$$+ A(rf'_x/2c^2) - Bu_xu_r/c^2 - Crf'_x/c^2],$$

where the quantities in round brackets ( ) are due to the expansion in series, i.e. to the finite velocity of propagation. If now we develop the functions A, B, C:

$$A = \alpha_0 + \alpha_1u^2/c^2 + \alpha_2u^2_r/c^2 + \ldots,$$
$$B = \beta_0 + \beta_1u^2/c^2 + \beta_2u^2_r/c^2 + \ldots,$$
$$C = \gamma_0 + \gamma_1u^2/c^2 + \gamma_2u^2_r/c^2 + \ldots,$$

we have

$$F_x/(ee'/r^2) = \cos(rx)[\alpha_0 + \alpha_1u^2/c^2 + \alpha_2u^2_r/c^2] - \beta_0u_xu_r/c^2$$
$$- r/2c^2 \,.\, [\{(3\alpha_0) - 2\alpha_0\}f'_r\cos(rx) + \{2\gamma_0 - (\alpha_0)\}f'_x], \qquad (11.4)$$

where the terms $(3\alpha_0)$ and (α_0) result from the expansion.

Since for two charges moving uniformly at relative rest ($\mathbf{u}=\mathbf{f}'=0$), this must reduce to Coulomb's law in elst measure, we have at once: $\alpha_0=1$. The other coefficients must be decided by experiment. Except that we cannot decide the form of the functions by general considerations, there is methodologically little difference between our present and our former procedure. Formerly we took $\varphi=e'/R(1-v_R'/c)$ as a generalisation of $\varphi=e'/r$, and similarly for the vector potential; we then arrived at a generalisation of Coulomb's law, and it remains for us to show that we can thereby explain the experimental results of electrodynamics within the limits of attainable accuracy. Now we proceed directly to generalise Coulomb's law in such a way that the propagation involved is ballistic, not medium-like; and it remains for us to investigate whether this entirely new law enables us to explain the experimental results at least equally well. This investigation, whether successful or not, is really of extraordinary interest; for it concerns the fundamental assumption of contemporary electromagnetic theory and confronts it once more with the older tradition.

Consider the quasi-stationary translational motion of a charged body of mass m, the electric densities being ρ at (xyz) and ρ' at $(x'y'z')$. We can calculate the action of the electrons on themselves from the formula [2]

$$mf_x=\iint\rho\rho' R_x d\tau d\tau',$$

where each pair of elements is taken twice. We can use formula (11.4). The electrostatic term gives zero since it satisfies the principle of action-reaction, the velocity-terms also give zero since all the elements have the same velocity. Hence from (11.4)

$$R_x=-[f_r'\cos(rx)+(2\gamma_0-1)f_x']/2c^2r$$

$$=-\frac{1}{2c^2r^3}[f_x'\{r_x^2+(2\gamma_0-1)r^2\}+f_y'r_xr_y+f_z'r_xr_z].$$

That is, the body experiences a force which is a linear function of the accelerations:

$$mf_x=-1/2c^2 \,.\, (A_xf_x'+B_xf_y'+C_xf_z'),$$

[2] That is, we express (11.4) in the form $F_x=R_x\,dede'$; and using ρ and ρ' for electric densities, we put $de=\rho d\tau$, $de'=\rho' d\tau'$.

where

$$A_x = \iint \rho\rho' d\tau d\tau' r^{-3}[(2\gamma_0 - 1)r^2 + (x - x')^2]$$

$$B_x = \iint \rho\rho' d\tau d\tau' (x - x')(y - y')/r^3$$

$$C_x = \iint \rho\rho' d\tau d\tau' (z - z')(x - x')/r^3.$$

These results are the same as hold for feeble velocities on the electron-theory of Lorentz (8.1a), provided $2\gamma_0 - 1 = 1$, i.e. $\gamma_0 = 1$. We shall therefore assume this value, which will be confirmed later. On Ritz's theory this anisotropic inertial reaction holds even for high velocities, for the relative velocity continues to be zero in translation. When the body is symmetrical, this reaction is parallel to **f**. In this case, taking x along f, we see that the quantity

$$m' = 1/2c^2 \, . \iint \rho\rho' d\tau d\tau' [r^2 + (x - x')^2]/r^3$$

plays the part of an electromagnetic mass added to the ordinary mass m. For a uniformly charged sphere of radius a, $m = 4q^2/5a$, where q is the charge in elms.

If in our approximation (11.4) we carried the development as far as terms in $1/c^3$, we should find an extra resultant force

$$\frac{2}{3}\frac{e^2}{c^3}\frac{d\mathbf{f}}{dt}. \tag{11.4a}$$

This force, exerted by a charge on itself, occurs also (for small velocities) in Lorentz's theory (8.11a). It is independent of the form of the body and represents a quasi-friction due to loss of energy by radiation.

This formula (11.4a) is so important, in view of our remarks on radiation in the previous chapter, that we shall add an explicit proof. Formulae (11.3a) now have an extra term :

$$x'(t - \rho/c) = x' - \rho v'_x/c + \rho^2 f'_x/2c^2 - \rho^3 g'_x/6c^3,$$
$$v'(t - \rho/c) = v'_x - \rho f'_x/c + \rho^2 g'_x/2c^2,$$

where the letters without added argument refer to the time t and $g'_x \equiv df'_x/dt$. And (11.3b) becomes

$$\rho_x = r_x + \rho^2 f'_x/2c^2 - \rho^3 g'_x/3c^3,$$
$$\rho^2 = r^2 + \rho^2 r f'_r/c^2 - 2\rho^3 r g'_r/3c^3.$$

Hence

$$\rho/r = 1 + rf'_r/2c^2 - r^2g'_r/3c^3,$$

$$\rho^{-2}\cos(\rho x) = r^{-2}\cos(rx)[1 - 3rf'_r/2c^2 + r^2g'_r/c^3] + f'_x/2c^2r - g'_x/3c^3,$$

$$1 - c^{-2}\rho f'_e(t - \rho/c) = 1 - rf'_r/c^2 + r^2g'_r/c^3.$$

Also

$$U_x \equiv v_x - v'_x(t - \rho/c)$$
$$= u_x + rf'_x/c - r^2g'_x/2c^2,$$
$$U\rho = \Sigma U_x \cos(\rho x)$$
$$= u_r + rf'_r/c - r^2g'_r/2c^2 + r(\mathbf{f'u})/2c^2.$$

Inserting these values in (11.3) we obtain the following extra terms in the expression F_x, i.e. in addition to those occurring in (11.4):

(1) . $$- de\, de'\beta(u_xf'_r + u_rf'_x)/c^3r,$$

which we take as negligible.

(2) $$de\, de'(C - A/3)g'_x/c^3.$$

In this latter put $C = A = 1$ and integrate, and we obtain (11.4a).

This result is very striking, for Ritz's force-formula was derived from very general ballistic considerations, without the smallest reference to radiation. If now, as we have to do in the case of the ordinary aether-electron theory, we associate this extra term with the transmission of radiation, we obtain agreement with the results discussed in the last chapter. In particular we obtain the correct result for radiation-pressure. This is the first successful achievement of Ritz's theory; but we shall find many more as we proceed.

Reverting to formula (11.4), we see that the acceleration terms can be divided into two parts. The first

$$ee'/2c^2r\,.\,[f'_x - 3f'_r\cos(rx)] \tag{11.4b}$$

is due to the development in series and therefore to the finite velocity of propagation. This, as we shall see, determines the phenomena of induction in a closed circuit; it also determines the electric force in the immediate neighbourhood of a Hertzian vibrator. The second part

$$ee'/c^2r\,.\,[-f'_x + f'_r\cos(rx)] = -ee'/c^2\,.\,\partial f'_r/\partial x \tag{11.4c}$$

is zero for a closed circuit. It corresponds to Fresnel's vector in optics.

These results follow because the acceleration terms are identical

in the second-order formulae of Liénard-Schwarzschild (7.17) and Ritz (11.4). We must now make a much more exacting test by comparing the two general formulae (7.14) and (11.3) for the case of electric oscillations, in which only the electrostatic and acceleration terms count, the velocities being relatively negligible as is the case for Hertzian vibrations. Formula (11.4) becomes

$$F_x/ee' = \frac{\cos(\rho x) - \rho f'_x/c^2}{\rho^2(1-\rho f'_\rho/c^2)}$$
$$= \frac{\cos(\rho x)}{\rho^2} - \frac{f'_x - f'_\rho\cos(\rho x)}{c^2\rho(1-\rho f'_\rho/c^2)}. \qquad (11.5)$$

We must now compare this with formula (7.21). We have

$$\rho_x = x - x' - \rho/c \,.\, v'_x = R_x - \rho/c \,.\, v'_x$$
$$\rho^2 = R^2 - 2\rho R v'_R/c + \rho^2 v'^2/c^2.$$

Hence, since v'/c is very small (of the order 10^{-10}), we can put

$$\rho = R(1 - v'_R/c)$$
$$\rho_x = R[\cos(Rx) - v'_x/c].$$

Hence the first term in (11.5) is

$$\frac{\cos(\rho x)}{\rho^2} = \frac{\rho_x}{\rho^3} = \frac{\cos(Rx) - v'_x/c}{R^2(1-3v'_R/c)}.$$

That is, it is sensibly identical with the first term in (7.21). Therefore the electrostatic terms are sensibly the same in the two theories.

Next consider the second terms. In Hertz's experiments the frequency n varied from 10^8 to 10^{11}, the maximum distance in which the waves were observed was about $m\lambda$, where $m = 100$. Hence, using square brackets to denote maximum values,

$$[\rho] = m\lambda = mc/n$$
$$[f'_\rho] < [f'] = 2\pi n[v'],$$

so that

$$[\rho f'_\rho/c^2] < 2\pi m[v'/c],$$

i.e. was of the order 10^{-9}, and could therefore be regarded as negligible in the second term of (11.5). Since the values and directions of ρ and R differ only by quantities of the order v'/c, i.e. 10^{-10}, we can replace ρ by R. That is the second term becomes

$$[f'_R\cos(Rx) - f'_x]/c^2R$$

as in (7.21) or (7.22). Ritz concludes as follows (p. 425):

> The new theory well represents Hertzian oscillations. The fictitious particles are then distributed periodically in time and space; this distribution in its turn excites oscillations of other ions or systems of ions; the combination of these actions by interference, i.e. by simple superposition, then gives rise to the various phenomena of reflection, refraction, etc. When we can consider the velocities of the ions and the amplitude of their accelerations as infinitely small, the agreement between my formulae and those of Lorentz, proved for Hertzian oscillations, continues to exist whatever be the frequency; with this restriction, both would represent the phenomena of optics.

This is a very striking achievement. It shows us clearly that the project of reviving a ballistic theory, which would have appeared so hopeless in the pre-electron heyday of Maxwell's equations, is very far from being fantastic or beneath scientific criticism. Ritz's attempt is only the pioneer blazing of a trail which physicists have without adequate reason neglected. There is as yet a limitation as regards optics, apart from a really critical examination of the experimental evidence once we begin to be genuinely open-minded. That consists in the term

$$\rho f'_\rho/c^2 = Rf'_R/c^2 - R\Sigma f'_x v'_x/c^3,$$

which Ritz shows to be negligible for Hertz's experiments.

This is the result as stated by Ritz. But in view of the conclusions at which we arrived in Chapter VIII, we can now take the comparison much further. We have rejected radiation by a single electron, and experiment decisively confirms this rejection. If this is correct, Ritz's exposition of Hertz's experiments is faulty. We must start with a statistical group of point charges, for which v/c is negligible. On Ritz's theory as on the ordinary theory, the radiation-reaction is $2e^2\dot{\mathbf{f}}/3c^2$; which gives a rate of energy-emission $2e^2f^2/c^3$. We have shown in Chapter VIII that this energy may be taken as distributed in the wave-shell with density w (8.67). We have also shown that it produces the correct radiation-pressure. It follows therefore that Ritz's theory is far more successful in optics than its inventor thought. And further admissions are made even by its opponents.

> What is emitted was of course not supposed to be material particles which obey mechanical laws, but an agent which, when it enters into matter, exerts directed transversal forces and sets it into vibration. Light-vibrations exist then only in matter and not in the ether. The objection that an emission theory is unable to

account for interference is clearly unjustified in the case of this view.—Born, *Einstein's Theory of Relativity*, 1924, p. 185.

The experimentally determined fact of waves capable of interference is not incompatible with this postulate [Ritz's theory], since it can be explained by admitting that positive and negative actions irradiate successively from a source.—Giorgi, *Atti del Congresso dei Fisici*, 2 (1928) 286.

Hence when Kennard says (iv. 172) that 'so far as the writer is aware, there is no rival to the Maxwell-Lorentz theory which explains all ordinary phenomena (including Hertzian waves) and also removes the torque on the condenser' in the Trouton-Noble experiment, he is merely echoing the complacency of contemporary physicists who treat the work of Gauss, Weber, Riemann and Ritz as negligible in the history and elaboration of electromagnetic theory. We are assured that 'the four fundamental electromagnetic equations of Maxwell are merely statements of elementary experimental facts which any schoolboy can now verify.'[2a] Yet, as we shall presently see, all these experimental facts are even better explained on Ritz's theory. The schoolboy may be forgiven for the dogmatism which has been injected into him. But surely it is high time for teachers and writers to pay serious attention to the challenge to their very modern prepossessions which has been before the scientific world since 1908. That is, twenty-eight years before Sir J. J. Thomson (xiv. 393) published the statement that 'this is the outstanding feature of Maxwell's theory, it is the only theory which tells us that the velocity of light is 3×10^{10} cm./sec.'

Now Ritz's contention may be wrong, but at least it should be tested. It has at least had this effect: it has antiquated and superseded most, if not all, of the results which have been alleged as proving the Maxwell-Lorentz theory. This follows from the fact—which will be demonstrated in the next chapter—that all of these results are equally, better in some cases, in accord with Ritz's formula.

It is the boast of present-day writers on physics, and especially of 'relativist' popularisers, that they have freed themselves from traditions and prejudices. And yet when we appraise them critically we discover that there are strict bounds to their freedom which, within the limits set by contemporary scientific orthodoxy, is so gleefully reckless. Our attempt is much more

[2a] Drysdale, *Nature*, 134 (1934) 796.

radical. For while we intend to apply the criterion of educated commonsense to the metaphysical exuberance of the present Einstein epoch, we also propose to lay hands on the secret shrines at which even the iconoclasts worship. In other words, we shall adopt a fair judicial attitude in the case of Ritz *versus* Maxwell.

2. The Electronic Theory of Conduction.

The present view of metallic conduction is that there is a drift-velocity of free electrons due to an impressed electric field, the atoms being treated as comparatively immobile.[3] In a sense this is a return to Franklin's one-fluid theory; he took vitreous electricity as a weightless fluid like Black's caloric, i.e. for him it was *positive* electricity which was alone mobile. Fechner (i. 337) expressed in 1845 the view that a current consisted of plus and minus electricities flowing with equal velocities in opposite directions. This was repeated as late as 1908 by L. Graetz who says (p. 818), that 'an electric current in a conductor consists of a double motion of both electricities.' But in 1871 Carl Neumann advocated the view that in metals, unlike electrolytes, only positive electricity moves:

> Each metal is to be regarded as composed of two kinds of material particles: (1) ponderable particles each of which is inseparably bound up with a certain quantity of negatively electric matter; (2) particles each of which consists only of positively electrical matter. Owing to their mutual cohesive forces, the first-named particles form a rigid (or elastic) network, which is properly the solid substance of the metal. But the second-named particles form a movable fluid within this solid substance.—C. Neumann, x. 394.

Clausius (i. 88, v. 228) upheld the same view. The accepted view to-day agrees with this in regarding one kind of electricity as immobile, but maintains that it is the *negative* electricity (i.e. the electrons) which move through the metal. Though the point may be regarded as elementary, it is important to show by a few typical quotations that it is accepted for reasons quite apart from the Weber-Ritz hypothesis:

[3] The electronic theory of metals was started by W. Giese, AP 35 (1888) 255 (French translation in Abraham-Langevin, p. 236) and especially by P. Drude, AP 1 (1900) 566, 3 (1900) 369 (French translation, p. 162). We are not here concerned with modern refinements and complications. *Cf.* R. Peierls, *Ergeb. d. Exakt. Naturwiss*, 11 (1932) 264–322; J. C. Slater, *Rev. of Mod. Physics*, vol. 6, no. 4 (October 1934). Nor are we concerned with the 'positive electron.'

Once the existence of electrons became known, it seemed natural to consider these free electrons as carriers of the electric current through the metal. This opinion, which had previously been put forward by W. Weber, was now revived and further developed by E. Riecke and P. Drude.—Planck, *Where is Science Going ?*, 1933, p. 49.

For the sake of simplicity we shall assume only one kind of free electrons, the opposite kind being supposed to be fixed to the ponderable matter.—Lorentz, ii. 63.

The motion of electric charges is called an electric current. . . . The freely moving carriers in metals are electrons.—Grimsehl-Tomaschek, pp. 135, 361.

The modern view of electricity regards a current of electricity as a material flow of electric charges. In all conductors, except a small class known as electrolytic conductors, these charged bodies are believed to be identical with the electrons.—Jeans, p. 306.

We regard the current . . . as a movement of the atoms of electricity through the conductor. . . . In metals it is only the electrons that move, the atoms of positive electricity keep their positions even in an electric field.—Pohl, pp. 41, 246.

To-day we can say with certainty that an electric current is nothing but a stream of small electrically charged particles : the ions in electrolytic conductors, the electrons in metallic.—Mie, p. 47.

Unfortunately the obvious conclusion is not drawn from this unanimously admitted premiss. It means of course the rejection of the view of Maxwell who says (ii. 157) : 'It must be carefully remembered that the mechanical force which urges a conductor carrying a current across the lines of magnetic force, acts, not on the electric current but on the conductor which carries it.' Writing in 1879, Hall [4] said : 'This statement seemed to me contrary to the most natural supposition in the case. . . . I brought the question to Prof. Rowland. He told me he doubted the truth of Maxwell's statement.' Hall in 1879 discovered a difference of potential between the two ends of a current-traversed plate when placed between the poles of a magnet. As Jeans says (p. 563), 'the Hall effect is of interest as exhibiting a definite point of divergence between Maxwell's original theory and the modern electron theory,' for it shows that the current consists of moving electricity which is displaced by a magnetic field, i.e. by another set of moving charges.

[4] Cited by L. L. Campbell, *Galvanomagnetic and Thermomagnetic Effects*, 1923, p. 6. 'A satisfactory explanation of the variation in signs of the Hall and allied effects waits upon the future.'—*Ibid.*, p. 94. 'No satisfactory explanation of the positive Hall coefficient has been proposed.'—Page-Adams, p. 296. Presumably the anomaly is explicable by hypotheses concerning the motion of the positive ions.

But the really vital deduction is the general conclusion that all electrodynamic phenomena must be explained statistically from a law of force between moving charges. As Fechner (i. 337) said in 1845 and Weber (i. 178) said in 1846, we must regard Ampère's formulae as depending primarily on the actions of electric particles *inter se* and only indirectly on the carriers or metallic conductors. As Wiechert (i. 562) said in 1900, 'following the procedure of W. Weber, we must resolve the electrodynamic effects of matter into contributions from the individual electrons.' That is, the fundamental formula of electromagnetics is the force-formula, whether that of Liénard or that of Ritz; and from it we must derive Ampère's formulae, induction and all the other experimental results, including wireless waves. Yet it is the extraordinary fact that not a single text-book attempts to do so, in this outdoing even Maxwell who at least gave Weber's derivation.[5] Bouasse (i. 133) even tells us that 'Ampère's formula presents only an historical interest.' Whereas the formula is really a correct representation of experimental results; and what is historically out-of-date and logically offensive is the almost universal failure to derive it from the professedly held electron theory.

Moreover, we have now got beyond the position of Maxwell who could write (v. 97): 'We are unable to determine whether the "velocity of electricity" in the wire is great or small.' We have returned to the position of Weber[6] that the velocity is very small, w/c being of the order 10^{-10} and therefore usually quite negligible. We can investigate this in an elementary approximate manner which suffices for our purpose. Suppose V volts to be the potential-difference and R ohms to be the resistance of a copper wire, A sq. cm. in cross-section, carrying a current J amps. If the current is due to N conductivity-electrons per cm.^{3}, each with a charge $-e$ coulombs and an average drift-velocity $-w$ cm./sec.,

$$V/R = J = NewA.$$

For a copper wire, 10 metres in length and $A = 0\cdot01$, $R = 0\cdot17$. As an approximation take $N =$ no. of atoms in 1 cm.^{3} of copper $= 85 \,.\, 10^{21}$; and we know $e = 1\cdot6 \,.\, 10^{-19}$. In practice we cannot

[5] So did Poincaré, but he hastens to add (iv. 263): 'Nothing is farther from my thought than to defend it.' Mathieu (p. 88) actually adopts Weber's law.

[6] See Zöllner, v. 525. In 1880 A. von Ettingshausen deduced from the Hall effect in gold that the velocity was about 0·1 cm./sec.—AP 11 (1880) 432.

maintain in copper a field-strength exceeding $V/l = E = 10^{-3}$ volt/cm.; and as $l = 10^3$, we have $V = 1$. Hence

$$w = V/RNeA$$
$$= 0\cdot043,$$

that is 1/23 cm. per sec. That this represents the right order of magnitude, is now universally admitted.

> For a current of 100 amperes per sq. cm. w is only $7\cdot5 \times 10^{-3}$ cm. per second. The considerable transfer of charge obtained in ordinary experiments is therefore due to the very large number of electrons concerned rather than to any great speed of each.—Pidduck, p. 606.
>
> In this case [copper wire] the electrons merely crawl through the copper conducting wires with a velocity of not more than about half a mm. per sec.—Pohl, p. 250.
>
> In copper the velocity of the conducting electrons is in practice much below 1 cm. per sec., and even in filaments of incandescent lamps it certainly does not reach 1 cm. per sec.—Boll, p. 22.
>
> Consider a conductor in which the number of electrons per c.cm. is 10^{21}. Then in a wire of 1 sq. mm. cross-section there are 10^{19} electrons per unit length, so that the average velocity of these when the wire is conveying a current of 1 amp. is of the order of 1 cm. per sec. This average velocity is superposed on to a random velocity which is known to be of the order of magnitude of 10^7 cm. per sec., so that the additional velocity produced by even a strong current is only very slight in comparison with the normal velocity of agitation of the electrons.—Jeans, p. 307.
>
> Drude originally considered the possibility of electric conduction in metals taking place on account of both positive and negative carriers. But so many facts have indicated that all the carriers are the negatively charged electrons, that we shall consider this conception alone. . . . Owing to the small mass of the electrons (about 1/1840 that of a hydrogen atom), their velocity must be very great if they behave as particles of an ideal gas; and calculations from the kinetic theory show that the velocity will be of the order 10^7 cm. per second. At normal current densities, the velocity of general drift of the electrons (w) caused by the electric force will therefore be very small compared with the normal gas velocity (v). Thus for copper at a current-density of 10^4 amps. per sq. cm., w is about 1 per cent. of v.—Hume-Rothery, *The Metallic State*, 1931, p. 167.
>
> Although immense numbers of electrons take part in the flow of currents through metallic conductors, the velocity of migration of the electrons never exceeds a fraction of a centimetre per second.—Pilley, p. 189.

It is as well to get this point clear at the outset, quite apart from any discussion of rival electron theories. In what follows

we shall therefore feel generally justified in neglecting w^2/c^2. We shall assume that there are positive and negative ions, the positive ions being attached to the matter and moving therewith at the velocity $\mathbf{v}$ at any point. The negative ions or electrons, n per unit length, move with the velocity $-\mathbf{w}$ relatively to the conductor, so that $-\mathbf{w}$ is along the element $\mathbf{ds}$ and the total velocity (relative to the axes or to the aether) of the electrons is $\mathbf{v}-\mathbf{w}$. In elst units the current is $j = n(-e)(-w) = new$. The charge on the element is $q = neds$, so that $qw = jds$. We take this charge to be neutralised by the charge $-q$ of the ions of opposite sign, so that, to use Ritz's term, the current is *neutral*. Even if we do not take the charge to be absolutely zero $(q-q)$, it is assumed to be the difference $(q_1 - q_2)$ of two much greater charges (q_1 and q_2); and as the relevant terms are small, containing the factor $1/c^2$, this will not sensibly affect most of the results.[7]

Maxwell's opposition to this view of an electric current was founded on scientific objections which seemed plausible enough in his time.[8] He describes (ii. 216–222) three inertia effects which should exist in conductors if an electric current is due to the motion of only one kind of electricity and if this possesses inertia.

(1) If a current in a circular coil free to move about its axis is altered, the free electricity will be accelerated and the coil itself will receive an equal and opposite change of momentum. Maxwell says (ii. 218):

> If any action of this kind were discovered, we should be able to regard one of the so-called kinds of electricity, either the positive or the negative kind, as a real substance; and we should be able to describe the electric current as a true motion of this substance in this direction. . . . There is as yet no experimental evidence to show whether the electric current is really a current of a material substance or a double current, or whether its velocity is great or

[7] Maxwell (ii. 482) incorrectly says: 'The quantity $[q_1-q_2]$ may be shown experimentally not to be always zero.' Fechner already in 1845 (ii. 339) spoke of 'the opposite electricities' of the neutral wire. Weber and Riemann took the same view, and indeed it is already implied when formula (4.31a) is applied to metallic conductors. This 'neutrality' renders invalid Maxwell's criticism (ii. 481 f.) of Weber, as Poincaré pointed out (iv. 263). The assumption ($q_1 - q_2$ small relatively to q_1 or q_2), he says, 'appears natural enough if we consider the velocity which certain physicists attribute to electricity in electrolytes.'

[8] See Barnett's article (xi.).

small as measured in feet per second. A knowledge of these things would amount to at least the beginnings of a complete dynamical theory of electricity.

As a matter of fact, Barnett (ix. 349), with the aid of the more sensitive appliances available to-day, has measured this effect. He finds that the charge of the carriers is negative and $e/m = 1 \cdot 8 \times 10^7$.

(2) A coil traversed by a steady current, so that the electricity has constant angular momentum, should exhibit the properties of a gyrostat. Maxwell in 1861 vainly sought for this effect. Barnett proved the effect for the individual whirls of Ampère (iv. 239), as has been mentioned in the last chapter.

(3) If the coil is accelerated about its axis, the free electricity will be differentially accelerated, lagging behind when the speed is increased and going ahead when the speed is lessened. Thus the acceleration of the coil gives rise to an electric current. This has been verified by Tolman [9] and his fellow-workers, e being found to be negative and $m = m_H/1910$.

Maxwell said (ii. 222) concerning these three tests :

> I have pointed them out with the greater care because it appears to me important that we should attain the greatest amount of certitude within our reach on a point bearing so strongly on the true theory of electricity.

His attitude was commendably scientific. Unlike many of his followers, he realised that subsequent experiment might considerably modify the views he propounded. Naturally he did not expect a return to the views of Weber.

In spite of the absence of confirmation of these three tests until quite recently, the electron theory forged ahead. That the tests were not lost sight of in the meantime is shown by this unpublished letter of G. F. FitzGerald dated 6 June 1900 (addressed to the late Prof. Orr) :

> What do you think of the pressure of electrons in a solid conductor? If the negative electron is free to move and the positive one fixed, ought not a conductor to experience a quite considerable kick when suddenly subjected to an electric force ? As for example when linked with a ring magnet that was suddenly magnetised. They say the electrons have nearly $\cdot 001$ of the mass of atoms. If this

[9] Tolman and Stewart, PR 8 (1916) 97 ; Tolman, Karrer and Guernsey, PR 21 (1923) 525 ; Tolman and Mott-Smith, PR 28 (1926) 794. Also Tolman and McRae, ' Experimental demonstration of the equivalence of a mechanically oscillated electrostatic charge to an alternating current.'—PR 34 (1929) 1075.

were so, would not there be quite a big kick on the conducting wire due to the sudden starting off of the free negative ones ? After all maybe they start off too slowly; still a very small kick could be observed if the conductor were hung by a quartz fibre; resistance would not stop the electrons for quite a sensible time. If corpuscles differ from electrons, then in a conductor there should be quite a sensible amount of the kinetic energy of a current in the form of motion of corpuscles. Hertz looked for this but he never found it. It would certainly be worth looking for, though I am doubtful whether you could distinguish it from an error in the calculated self-induction due to the electrons not being of sensible size, i.e. to the current not being uniformly distributed throughout the conductor.

3. Forces between Linear Circuits.

Let us apply this analysis to find the force between two current-carrying circuits. At ds (Fig. 51) we have q moving with $\mathbf{v}$ and $-q$ with $\mathbf{v}-\mathbf{w}$, at ds' we have q' moving with $\mathbf{v}'$ and $-q'$ with $\mathbf{v}'-\mathbf{w}'$. Let us first assume that these are referred to the aether and apply the Liénard-Schwarzschild formula

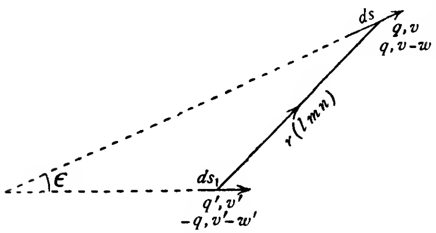

Fig. 51.

(7.17), remembering that there are four forces exerted by $\mp q'$ on $\mp q$. Consider the various terms :

$$\Sigma ee' = 0$$

$$\Sigma ee'v'^2 = (q-q)q'v'^2 + (q-q)(\mathbf{v}'-\mathbf{w}')^2 = 0$$

$$\Sigma ee'v_r'^2 = 0$$

$$\begin{aligned}\Sigma ee'(v_x v'_x + \ldots) &= qq'v_x v'_x + q(-q')v_x[v'_x - w'\cos(xds')] \\ &\quad - qq'[v_x - w\cos(xds)_{\,}\iota_x \\ &\quad + qq'[v_x - w\cos(xds)][v'_x - w'\cos(xds')] \\ &\quad + \ldots \\ &= qq'ww'\Sigma\cos(xds)\cos(xds') \\ &= jj'dsds'\cos\varepsilon \text{ in elsts}\end{aligned}$$

$$\begin{aligned}\Sigma ee'v'_x v_r &= q'v'_x q\Sigma l v_x - q'[v'_x - w'\cos(xds')]\Sigma l[v_x - w\cos(xds)] \\ &\quad + q'v'_x(-q)\Sigma l[v_x - w\cos(xds)] \\ &\quad - q'[v'_x - w'\cos(xds')]q\Sigma l v_x \\ &= qq'ww'[\Sigma l\cos(xds)]\cos(xds') \\ &= jj'dsds'\cos(rds)\cos(xds').\end{aligned}$$

The acceleration term gives zero.

Inserting the results in (7.17), we obtain

$$\begin{aligned} d^2F_x &= \Sigma ee'/r^2 \,.\, [-\cos(rx)(v_x v'_x + \ldots)/c^2 + v'_x v_r/c^2] \\ &= JJ'dsds'/r^2 \,.\, [-\cos(rx)\cos\varepsilon + \cos(rds)\cos(xds')], \quad (11.6) \end{aligned}$$

where $J = j/c$ and $J' = j'/c$ are the elm current-measures.

Integrating over the complete circuit s', we have

$$dF_x = JJ'ds \int ds' r^{-2}[\cos(xds')\cos(rds) - \cos(rx)\cos(dsds')].$$

Which is Ampère's formula (4.6).

Formula (11.6) can be written vectorially

$$\begin{aligned} \mathbf{d^2F} &= JJ'r^{-3}[\mathbf{ds'}(\mathbf{rds}) - \mathbf{r}(\mathbf{dsds'})] \\ &= JJ'r^{-3}V\mathbf{ds}V\mathbf{ds'r}. \qquad (11.6a) \end{aligned}$$

Hence, for a complete circuit s', we obtain formula (4.5) for the force exerted by a magnetic field :

$$\mathbf{dF} = JV\mathbf{dsH'},$$

where

$$\begin{aligned} \mathbf{H'} &= J' \int V\mathbf{ds'r}/r^3 \\ &= -J' \int \mathbf{ds'}\nabla_0 \frac{1}{r} \\ &= \mathrm{curl}_0\, \mathbf{A'} \end{aligned}$$

where $\mathbf{A'}$ is $J' \int \mathbf{ds'}/r$.

Generalising this result, we assume that any magnetic field is due to a series of closed uniform neutral currents.

In deducing Ampère's formula from the force-formula, as an effect valid to the second order, we have taken the second step—the first being Hertz's results—in a synthetic exposition of electromagnetics based on the aether-electron theory. We have already seen, and it is easy to verify, that the force-formula of Clausius (7.20) also gives Ampère's formula ; but Clausius fails to account for Hertz.

We have now proved the point to which we referred in Chapter IV, namely, that the accepted theory of electromagnetics gives formula (11.6a = 4.12d). It follows that any objections to this formula will have a far-reaching effect ; for they are really objections to the orthodox theory which even relativists accept.

Let us now turn to Ritz's second-order force-formula (11.4), taking the axes of reference to be any Newtonian set. The terms are evaluated as follows :

$$\Sigma ee' = 0$$

$$\begin{aligned}\Sigma ee'u^2 &= qq'(v_x - v_x')^2 - qq'(v_x - v_x' + w_x')^2 \\ &\quad - qq'(v_x - v_x' - w_x)^2 + qq'(v_x - v_x' - w_x + w_x')^2 + \ldots \\ &= -2qq'\Sigma w_x w_x' \\ &= -2jj'dsds' \cos \varepsilon\end{aligned}$$

$$\begin{aligned}\Sigma ee'u_r^2 &= -2qq'w_r w_r' \\ &= -2jj'dsds' \cos(rds) \cos(rds')\end{aligned}$$

$$\begin{aligned}\Sigma ee'u_x u_r &= qq'(v_x - v_x')(v_r - v_r') - qq'(v_x - v_x' + w_x)(v_r - v_r' + w_r') \\ &\quad - qq'(v_x - v_x' - w_x)(v_r - v_r' - w_r) \\ &\quad + qq'(v_x - v_x' - w_x + w_x')(v_r - v_r' - w_r + w_r') \\ &= -qq'(w_x w_r' + w_r w_x') \\ &= -jj'dsds'[\cos(xds)\cos(rds') + \cos(rds)\cos(xds')]\end{aligned}$$

$$\Sigma ee'f_r' = \Sigma ee'f_x' = 0.$$

Hence

$$\begin{aligned}d^2F_x &= \Sigma ee' \cos(rx) \,.\, /r^2 \,.\, (\alpha_1 u^2/c^2 + \alpha_2 u_r^2/c^2) \\ &\qquad\qquad - \beta_0 \Sigma ee' u_x u_r/c^2 r^2 \\ &= -JJ'dsds'[R\cos(rx) + S\cos(xds) + S'\cos(xds')]\end{aligned}$$

where

$$\begin{aligned}Rr^2 &= 2\alpha_1 \cos \varepsilon + 2\alpha_2 \cos(rds)\cos(rds') \\ Sr^2 &= -\beta_0 \cos(rds') \\ S'r^2 &= -\beta_0 \cos(rds). \qquad (11.6b)\end{aligned}$$

Comparing these results with formulae (4.12), which give the most general expression compatible with Ampère's law, we find, substituting λ for k,

$$\alpha_1 = (3 - \lambda)/4, \quad \alpha_2 = -3(1 - \lambda)/4, \quad \beta_0 = (1 + \lambda)/2.$$

Putting $\alpha_0 = \gamma_0 = 1$ in (11.4), we may therefore rewrite Ritz's second-order force-formula in the form

$$\begin{aligned}F_x/(ee'/r^2) &= \cos(rx)[1 + (3 - \lambda)/4 \,.\, u^2/c^2 - 3(1 - \lambda)/4 \,.\, u_r^2/c^2] \\ &\quad - (1 + \lambda)/2 \,.\, u_x u_r/c^2 - r/2c^2 \,.\, [f_x' + f_r' \cos(rx)]. \qquad (11.7)\end{aligned}$$

It follows that, provided one of the circuits is closed, the Ritz formula gives all the results previously obtained from the Liénard

formula. The result in both cases is independent of the motion or deformation of the circuits provided : j and j' remain uniform, the element jds is neutral, the circuit j' is closed and neutral. The experimentally accessible facts concerning circuits and magnets, so far as hitherto examined, are equally explicable by either Lorentz's or Ritz's electron theory. We are now in a position to give a definite answer to the inquiry started by Maxwell in 1862:

> I want to see if there is any evidence from the mathematical expressions as to whether element acts on element, or whether a current first produces a certain effect in the surrounding field, which afterwards acts on any other current. Perhaps there may be no mathematical reasons in favour of one hypothesis rather than the other. . . . The theory of the effect taking place through the intervention of a medium is consistent with fact and to me appears the simplest in expression ; but I must prove either that the direct action theory is completely identical in its results or that in some conceivable case they may be different.—Campbell-Garnett, p. 331.

Maxwell is incorrect in the way he contrasts far-action and near-action theories. For we are now confronted with *two* near-action electron-theories, one based on medium and the other on ballistic transmission. Both give second-order simultaneous force-formulae, involving both distance and velocities (either absolute or relative). When integrated over a complete circuit both formulae give identical results, namely, the formulae found by Ampère well over a century ago. In this domain there is accordingly no evidence from 'the mathematical expressions' to distinguish between the two theories.

But once we admit that the complete closed circuit is not a primary entity, that the phenomenon is essentially one of moving electrons, we can no longer reject the law of force between current elements as devoid of physical significance. We cannot of course accept such a pronouncement as that made by Watson and Burbury (ii. 203) in 1889 :

> When we speak of a force acting between two current elements, we must be understood as meaning a force acting between the elementary conductors in which the currents flow, in virtue of those currents ; for we cannot conceive electric currents as in any other sense the subject of mechanical action.

But we must also reject such statements as the following :

> This dissection of the integral is allowable as a mathematical operation, . . . it has no general physical meaning.—Voigt, *Kompendium der Physik*, 2 (1896) 231.

The analysis into current-elements is only ideal.—P. Hertz, ii. 120.

The method of the element of a circuit is a purely fictitious one, since an element of a circuit cannot exist alone.—Starling, p. 52.

Strictly speaking, there is no such thing, from the Maxwellian point of view, as mutual action between current-elements . . . The mutual action of two German or irrational current-elements is indeterminate, and so we get a large number of so-called theories of electrodynamics.—Heaviside, ii. 500.

The mutual actions of a closed uniform current and a magnet, the actions of a magnet on a segment, great or small, of a current, are quantities which really exist; they can be observed and measured. But the action of an element of current on a magnet cannot be observed; such an action has no real existence; it is merely a mathematical fiction which serves as intermediary for calculating the action of a closed uniform current on a magnet.—Duhem, iii. 449.

The logical validity of the older electrodynamics was confined to systems of closed currents streaming round closed paths; and all investigations purporting to deduce from experimental data expressions for the electromotive forces induced in open circuits, or for mechanical forces acting on separate portions of circuits carrying currents, were necessarily illusory from the fact that such portions were practically unknown as separate independent entities.—Larmor, i. 21.

Each of those elements when regarded as a separate unit corresponds to an unclosed electric current, whereas on the modern theory of electricity such currents do not exist. Thus the mathematical unit does not correspond to a physical reality.—J. J. Thomson, ii. 277.

These statements, which are still in vogue, are replete with manifold misunderstandings.

(1) The fundamental answer is that the integral can no longer be regarded an anything but a summation of individual effects, so that according to the electron-theory it is the elements or members of this summation or superposition which constitute the primary phenomenon.

(2) A current-element therefore, consisting of q moving with $\mathbf{v}$ and $-q$ moving with $\mathbf{v}-\mathbf{w}$, cannot be branded as a mere mathematical fiction dissected out of a unitary integral phenomenon. What is true is that, in applying dynamical laws, we must deal with the constituent moving charges (q and $-q$); we cannot legitimately operate with ds except with such fictional safeguards as have already been indicated in Chapter IV.

(3) On the electron theory we can no longer admit that all currents are closed. We have already shown the impossibility

(it ought to be obvious !) of regarding $\dot{\mathbf{E}}/4\pi$ as a current, except by a quibble on the word 'electricity.' The correct phraseology centres round the retarded potential. Besides, the fact that Clausius's formula gives the correct result, shows that the retardation-effect is eliminated.

(4) It is quite true that we cannot isolate an element of a given closed circuit and find the force it exerts on another circuit or magnet. The element is there nevertheless, and contributes its quota ; and the formula we apply to it can be, and is, employed in isolation, e.g. for a single moving charge. Nor is it true to say with Heaviside that by dealing with the element we get an indeterminate number of electrodynamic theories or formula. So far we have only two : (*a*) Liénard-Lorentz and (*b*) Ritz. That of Clausius is merely a defective form of (*a*) without the propagation which we know *aliunde* to exist ; and we have already shown that, on the aether-electron theory, a certain constant which might supposedly be injected is necessarily unity as Lorentz takes it. As to (*b*) it is true that there is a constant λ, not so far determined, which is eliminated only by integration. Weber put $\lambda = -1$, Riemann took $\lambda = +1$; but these are arbitrary guesses. Though we cannot determine the constant by *these* experiments, it would be absurd to suppose that it is indeterminate ; we shall afterwards submit some evidence to show that $\lambda = 3$. Ultimately therefore we have the choice between two electron-formulae.

Even at this stage there emerges an objection to the Lorentz formula, an objection which has already been pointed out in connection with (7.17). According to this theory the force exerted by ds' on ds is proportional to $R \cos (rds) + S' \cos (xds')$, while that exerted by ds on ds' is proportional to $-[R \cos (rds') + S \cos (xds)]$; the two forces are not equal and opposite, it is only when we integrate that the unbalanced components are eliminated. We have already shown in Chapter IV the possibility of testing this experimentally.

4. From Weber to Ritz.

Let us begin by quoting what Ampère wrote in 1823 (pp. 96 f., 99) :

> We must conclude that these phenomena are due to the fact that the two electric fluids continually traverse the conducting wires,

> with a very rapid motion, uniting and separating alternately in the intervals between the particles of the wires. . . . When we suppose that [the electric molecules], set into motion in the conducting wires by the action of the battery, continually change their position, at each instant uniting to form a neutral fluid, then separating again, then going to join other molecules of the opposite fluid, it is no longer contradictory to admit that from the actions proportional to the inverse square of the distance which each molecule exerts, there can result between two elements of conducting wires a force which depends not only on their distance but also on the directions of the two elements. . . . If, starting from this consideration, it were possible that the mutual action of two elements is in fact proportional to the formula by which I have represented it, this explanation of the fundamental fact of the entire theory of electrodynamic phenomena should evidently be preferred to any other. But it would require investigations for which I lack time, as well as the even more difficult investigations which would be necessary in order to decide if the contrary explanation, according to which electrodynamic phenomena are attributed to motions impressed on the aether by the electric currents, can lead to the same formula.

It is clear from this that Ampère not only accepted 'electric molecules,' but also envisaged an explanation of his formula by resolution of the total effect into contributions from these molecules. Progress in this direction was slow. 'As yet,' wrote Fechner (ii. 337) in 1845, 'Faraday's phenomena of induction and the electrodynamic phenomena of Ampère have been related only by an empirical rule.' And Weber said in 1848 (Taylor, v. 489):

> A quarter of a century has elapsed since Ampère laid the foundation of electrodynamics. . . . All the advances which have since been really made have been obtained independently of Ampère's theory; as for instance the discovery of induction and its laws by Faraday.

In 1835 the first advance was made by Gauss (iv. 616) when he wrote in a note first published in 1867: 'Two elements of electricity in relative motion repel or attract one another differently when in motion and when in relative rest.' He arrived at a formula which, omitting the acceleration terms and putting $\lambda = -1$, is identical with Ritz's approximate formula (11.7). In a letter sent to Weber in 1845 (already quoted, p. 226), he based his view on non-instantaneous ballistic transmission. It is therefore an interesting historical fact that the paternity of Ritz's theory is ultimately traceable to Gauss.

But it was Wilhelm Weber who first developed Gauss's idea. Fechner declared (ii. 338) in 1845 that Faraday's induction and Ampère's electrodynamics could be correlated by means of the following two principles :

(1) Every action of a current-element can be regarded as compounded of the actions of a positive and equal negative electrical particle, which simultaneously traverse the same space-element in opposite directions.

(2) By this means the mutual action of two current-elements can be represented, with the assumption that like electricities attract if they are moving in the same sense and unlike electricities when moving in opposite directions.

This was the germ of the idea which Weber quantitatively worked out. In 1846 (i. 135) he declared that the laws of Ampère and Faraday 'are regarded as electrical, i.e. the forces are actions of electrical masses on one another,' which is exactly the point of view we hold to-day as against Maxwell's. In 1848 he initiated the greatest synthetic contribution to electrical science by his theory of 'the connection of the fundamental principles of electrodynamics with that of electrostatics' (Taylor, v. 510). Speaking of the laws of Coulomb and Ampère, he enunciated the view, accepted nowadays in the electron theory, that 'an inner connection between the two formulae can be found only by reverting to the treatment of the electric masses in the current-elements and their action.' Our present treatment of currents is, as Larmor said in 1897 (ii. 627), 'a final development of the Weberian notion of moving electric particles.' But at that time and for long afterwards it was accepted as a fundamental postulate that, to quote Carl Neumann (viii. 238), 'the mutual action of two bodies must always depend only on their relative relationships.' Those were the days when men were relativists, without talking about it ; nowadays we call ourselves relativists and uphold absolute velocities.

So in 1848 Weber enunciated his second-order generalisation of Coulomb's law and deduced therefrom the formulae of electrodynamics and induction :

$$F_x = ee' r^{-2} \cos (rx) [1 + r\ddot{r}/c^2 - \dot{r}^2/2c^2).$$

Now

$$dr/dt = v_r - v_r' = u_r$$
$$d^2r/dt^2 = d/dt \,.\, \Sigma(v_x - v_x')(x - x')/r$$
$$= (u^2 - u_r^2)/r + f_r - f_r'.$$

Hence Weber's formula is

$$F_x/(ee'/r^2) = \cos(rx)[1 + u^2/c^2 - 3u_r^2/2c^2 + r(f_r - f_r')/c^2], \quad (11.8)$$

which differs from Ritz's formula (11.7), with $\lambda = -1$, only in the acceleration terms. The difference, however, is vital. Helmholtz [10] urged serious objections to Weber, based on the occurrence of f_r in his formula; these objections do not apply to Ritz.

Consider the function

$$\begin{aligned} L/ee' &= (1 + u_r^2/2c^2)/r \qquad (11.9) \\ &= [1 + (v_r^2 + v_r'^2 - 2v_r v_r')/2c^2]/r \\ &= 1/r + [(\Sigma l v_x)^2 + v_r'^2 - 2v_r'\Sigma l v_x]/2c^2 r, \end{aligned}$$

where $(l\ m\ n)$ are the direction-cosines of r.

$$\begin{aligned} \partial L/\partial v_x &= ee'[l^2 v_x + l(m v_y + n v_z) - v_r' l]/c^2 r \\ &= ee' l(v_r - v_r')/c^2 r \\ &= ee' l u_r/c^2 r = ee' l \dot{r}/c^2 r \end{aligned}$$

$$\begin{aligned} \frac{d}{dt}\frac{\partial L}{\partial v_x} &= \frac{ee'l}{c^2} \cdot \frac{r\ddot{r} - \dot{r}^2}{r} + \frac{ee'\dot{r}}{c^2 r}\frac{d}{dt}\frac{x - x'}{r} \\ &= ee' \cos(rx)\,[u^2 - 2u_r^2 + r(f_r - f_r')]/c^2 r^2 \\ &\qquad + ee' u_r u_x/c^2 r^2 - ee' u_r^2 \cos(rx)\,.\,/c^2 r^2. \end{aligned}$$

Since

$$\begin{aligned} \frac{\partial u_r}{\partial x} &= \frac{\partial}{\partial x}\frac{\Sigma(v_x - v_x')(x - x')}{r} \\ &= \frac{v_x - v_x'}{r} - \frac{\Sigma(v_x - v_x')(x - x')}{r^2}\cos(rx) \\ &= u_x/r - u_r \cos(rx)\,.\,/r^2, \end{aligned}$$

we have

$$\begin{aligned} \partial L/\partial x = -ee' \cos(rx)[1 + u_r^2/2c^2]/r^2 &+ ee' u_x u_r/c^2 r^2 \\ &- ee' u_r^2 \cos(rx)\,.\,/c^2 r^2. \end{aligned}$$

Hence Weber's formula (11.8) can be expressed in the form [11]

$$F_x = -\frac{\partial L}{\partial x} + \frac{d}{dt}\frac{\partial L}{\partial v_x}$$

where $L = U - V, \quad U = ee'/r, \quad V = -ee'u_r^2/2c^2 r.$ (11.10)

[10] See also C. Neumann, MA 13 (1878) 571; Wiedemann, iv. 844. A force on P depending on the accleration of P is incompatible with Newtonian mechanics.—A. Przeborski, CR 197 (1933) 300.

[11] *Cf.* Weber, AP 73 (1848) 229.

Riemann (ii. 326) suggested an alternative :

$$L = ee'(1 + u^2/2c^2)/r, \tag{11.11}$$

which gives

$$\begin{aligned} F_x &= -\frac{\partial L}{\partial x} + \frac{d}{dt}\frac{\partial L}{\partial v_x} \\ &= ee'/r^2 \,.\, [\cos(rx)(1 + u^2/2c^2) - u_x u_r/c^2 + r(f_x - f'_x)/c^2]. \end{aligned} \tag{11.12}$$

This, except for the acceleration terms, is the same as Ritz's approximate formula (11.7) with $\lambda = +1$.

As far back as 1890, M. Lévy (i. 547) suggested the combination $(1-\alpha)L_1 + \alpha L_2$ as the 'potential' for gravitation, where the suffix 1 refers to Weber and 2 to Riemann. Similarly Ritz pointed out (p. 385) that his expression for the force (11.7) could be re-arranged to give

$$\begin{aligned} F_x = {} & (1-\lambda)/2 \,.\, F_{1x} + (1+\lambda)/2 \,.\, F_{2x} \\ & + ee'\, \lambda[f'_x - f'_r \cos(rx)] \\ & - ee'\, [(1+\lambda)/2 \,.\, f_x - (1-\lambda)/2 \,.\, f_r \cos(rx)]/c^2 r. \end{aligned}$$

Hence putting

$$\begin{aligned} L &= (1-\lambda)/2 \,.\, L_1 + (1+\lambda)/2 \,.\, L_2 \\ &= ee'/r \,.\, [1 + (1-\lambda)u_r^2/4c^2 + (1+\lambda)u^2/4c^2], \end{aligned} \tag{11.13}$$

we have

$$\begin{aligned} F_x = {} & -\frac{\partial}{\partial x}\left(L - \frac{ee'\lambda f'_r}{2c^2}\right) + \frac{d}{dt}\frac{\partial L}{\partial v_x} \\ & - \frac{ee'}{c^2 r}\left[\frac{1+\lambda}{2} f_x + \frac{1-\lambda}{2} f_r \cos(rx)\right]. \end{aligned} \tag{11.14}$$

After thus outlining Ritz's development of Weber's theory, combined with one of the two important ideas expressed by Riemann, it will be useful to review briefly some of the phases of the prolonged opposition to the general idea involved in Weber's view. Our object is not the same as that of Boltzmann who, in spite of his own tenacious fight for atomistic conceptions, wrote (iii. 349) [12] :

[12] *Cf.* Helmholtz (v. p. ix) : 'In Germany at that time [1878] the laws of electromagnetics were deduced by most physicists from the hypothesis of W. Weber. . . . So at that time the domain of electromagnetics had become a pathless wilderness.'

> The hypothesis of electric fluids was brought to high perfection by Wilhelm Weber, and the general recognition given to his work in Germany stood in the way of the study of Maxwell's theory. . . . It is certainly useful if Weber's theory is held up for ever as a warning example that we should always preserve the required mental elasticity.

Here it is the slowness of the supersession of Weber's ideas, at least in Germany, that is deplored; the fact that they were not abandoned quicker is held up to us as a warning example. Whereas our object is rather to learn a useful lesson from the complete abandonment of Weber's ideas, from their subsequent resuscitation in the form of an illogical amalgam of the electron theory with triumphant Maxwellianism, and from the present position which enables us to see that practically all the arguments against Weber are invalid. The rather superior and slightly contemptuous attitude once adopted towards Maxwell's predecessors—an attitude by no means extinct—can be best realised by a quotation from Tait's review of Maxwell's *Treatise*:

> The researches of Poisson, Gauss, etc., contributed to strengthen the tendency to such modes of representing the phenomena; and this tendency may be said to have culminated with the exceedingly remarkable theory of electric action proposed by Weber. All these very splendid investigations were however rapidly leading philosophers away towards what we cannot possibly admit to be even a bare representation of the truth. It is mainly owing to Faraday and W. Thomson [Kelvin] that we owe our recall to more physically sound, and mathematically more complex at least if not more beautiful, representations. The analogy pointed out by Thomson between a stationary distribution of temperature in a conducting solid and a statical distribution of electric potential in a non-conductor, showed at once how results absolutely identical in law and in numerical relations could be deduced alike from the assumed distance-action of electric particles and from the contact-passage of heat from element to element of the same conductor. . . . It is well-nigh twenty years since he [Maxwell] first . . . used (instead of Thomson's heat-analogy) the analogy of an imaginary incompressible liquid without either inertia or internal friction, subject however to friction against space and to creation and annihilation at certain sources and sinks. . . . In this paper Maxwell gave, we believe for the first time, the mathematical expression of Faraday's electrotonic state and greatly simplified the solution of many important electrical problems.—*Nature*, 7 (1873) 478.

The atomistic-statistical view is here curtly dismissed as failing to provide 'even a bare representation of the truth' for 'philosophers.' Even Poisson's analysis is rejected in favour of

Maxwell's 'analogy of an imaginary incompressible liquid.' As we have already pointed out, a large factor in this opposition was the anti-atomistic bias propagated by Maxwell's work. There were two or three other factors, the first of which was the alleged violation of the energy-principle by any such law as Weber's.

In 1855 Maxwell (iii. 208) gave very high praise to Weber's theory :

> Here then is a physical theory, satisfying the required conditions better perhaps than any yet invented and put forth by a philosopher whose experimental researches form an ample foundation for his mathematical investigations.

But he added what he considered a very serious objection :

> There are objections to making any ultimate forces in nature depend on the velocity of the bodies between which they act. If the forces in nature are to be reduced to forces acting between particles, the principle of Conservation of Force requires that these forces should be in the line joining the particles and functions of the distance only.

In his paper on the 'Dynamical Theory of the Electromagnetic Field' (1864), Maxwell expressed this objection much more forcibly :

> From the assumptions of both these papers we may draw the conclusions : first, that action and reaction are not always equal and opposite ; and second, that apparatus may be constructed to generate any amount of work from its own resources. . . . I think that these remarkable deductions from the latest developments of Weber and Neumann's theory can only be avoided by recognising the action of a medium in electrical phenomena.

His biographers Campbell and Garnett (p. 551) quote this as 'a good specimen of Maxwell's humorous irony.' It would have been more accurate to have pointed it out as one of the mistakes made by great men under the influence of strong preposession. The objection is further emphasised in a famous passage (§ 385) of Thomson and Tait's *Treatise on Natural Philosophy*, 1 (1867) 311 :

> A good type of such a theory is that of Weber, which professes to supply a physical basis for Ampère's theory of electrodynamics, just mentioned as one of the admirable and really useful third class. Ampère contents himself with experimental data as to the action of closed currents on each other ; and from these he deduces mathe-

matically the action which an element of one current ought to exert on an element of another, if such a case could be submitted to experiment. This cannot possibly lead to confusion.

But Weber goes further; he assumes that an electric current consists in the motion of particles of two kinds of electricity moving in opposite directions through the conducting wire; and that these particles exert forces on other such particles of electricity, when in relative motion, different from those they would exert if at relative rest. In the present state of science this is wholly unwarrantable, because it is impossible to conceive that the hypothesis of two electric fluids can be true; and besides because the conclusions are inconsistent with the Conservation of Energy, which we have numberless experimental reasons for receiving as a general principle in nature. It only adds to the danger of such theories when they happen to explain further phenomena, as those of induced currents are explained by that of Weber.

Accordingly, Weber's theory is classed among those 'which, however ingenious, must be regarded as in reality pernicious rather than useful.' It is well for us to remember to-day that this argument was sponsored by the then powerful authority of Kelvin and Tait. Zöllner was exceedingly courageous when he said of this passage (viii., p. lviii) that he had never met 'such a plethora of absolute nonsense in the short space of thirty lines.' In 1868 Tait renewed the attack on Weber, including Lorenz and Riemann in his sweeping condemnation:

On this theory of the electromagnetic field, Maxwell has attempted to found an electromagnetic theory of light. He determines from the equations representing the known laws of electricity the rate of propagation of any kind of disturbance. The physical quantities involved in this calculation have been already determined by W. Weber. . . . This remark of Maxwell's is of very great importance, giving us apparently an insight into the real connection between electricity and light. Riemann seems to have hit upon the same conclusion in 1858, but his paper was not published till 1867. Lorenz also has obtained a similar result [1867]. But the investigations of these authors are entirely based on Weber's inadmissible theory of the forces exerted on each other by *moving electric charges*, for which the conservation of energy is not true; while Maxwell's result is in perfect consistence with that great principle.—Tait, *Sketch of Thermodynamics*, 1868, § 132, p. 75 f.

The unfairness of this appeal to the energy-principle lies in the fact that as far back as 1848 Weber had given the formula for $L = U - V$. Which, as we have seen (4.73), is equivalent

$$T + U + V = \text{constant}.$$

In 1871 Carl Neumann wrote an article (x. 393) 'once and for all to refute the objection that Weber's theory is in contradiction with the principle of energy.' The replies began to have some effect, and in his *Treatise* (ii. 484) Maxwell admitted that the objection from the conservation of energy 'does not apply to the formula of Weber.' And now we must point out one of the ironies of history. So far from being a merit, this really constitutes a serious defect in Weber's theory; for it involves the occurrence of the acceleration f. Ritz's theory avoids this defect; his equation (11.14) is contrary to the conservation of energy, precisely because acceleration means radiation of energy.

This objection having been refuted—wrongly, as a matter of fact—other difficulties were raised. In 1877 Tait, having in the meantime read Weber's paper of 1848, wrote as follows:

> In 1848 Weber pointed out that his very remarkable law of electric attraction does give a potential: in the sense that the electric force in any direction upon a particle of electricity is the rate of diminution (per unit of length in that direction) of a certain function. It follows that when the system has been brought back to its original configuration and its original velocities, no work on the whole has been done. Clerk Maxwell has shown that it is on this account that Weber's theory is consistent with the production of induced currents. But this potential involves *relative velocities* as well as relative positions, and cannot therefore be properly called potential energy.-Tait, *Sketch of Thermodynamics*, 1877[2], p. 69 note.

We have here the remarkable statement that absolute velocities, i.e. velocities relative to a medium or framework, are necessary in electromagnetics because something called 'potential energy' so requires. The idea still survives to-day; but as an argument it is mere verbiage. The question is one to be exclusively decided by experiment.

The next objection has also persisted and exercised great influence against Weber's views. It is thus expressed by Tait:

> (*a*) Matter consists of ultimate particles which exert upon each other forces whose directions are those of the lines joining each pair of particles and whose magnitudes depend only on the distances between the particles. . . . Unfortunately it must be confessed that we know nothing as to the ultimate nature of matter, and (*a*) is not in the present state of experimental science more than a very improbable [probable ?] hypothesis. . . . The best-known complete hypothesis (that of Weber) on which the mutual actions of electric currents have yet been explained, requires the admission of mutual forces between moving quantities of electricity, which are

not consistent with (*a*).[13] But before the *facts* discovered by Joule, all such objections must give way. . . . Our real difficulty in such a case as this is not with regard to the truth of the Conservation of Energy, but with regard to the *nature of electricity*; and Weber's result merely shows that electricity does not consist of two sets of particles, vitreous and resinous, not that there is a loophole for escape from the grand law of Energy.—Tait, *Sketch of Thermodynamics*, § 96, 1877², p. 68 f.

It is here objected that the law of force between particles must in all cases be a function of their—presumably simultaneous—mutual distance, and cannot involve velocity either relative or absolute. This is another example of *a priori* dogmatism. For its refutation it is sufficient to point out, as we have already done, that the selfsame criticism was then and is now applicable to the theory held by Maxwell, Tait and their successors of to-day. For, in the light of the electron theory, we know that the accepted theory of electromagnetics is reducible to Liénard's fundamental force-law. As far as the electromagnetic phenomena explained by Weber are concerned, this later force-law can be expressed in a form (7.17) involving not only the simultaneous distance of the particles but also their velocities relative to the aether. We here encounter another irony of history in the fact that the prevalent theory of to-day is itself subject to the identical criticism which was once considered decisive against Weber.

The objection which has persisted longest—it is still repeated in our text-books—is that a law of the Weber-Riemann-Ritz type is based on *actio in distans* and implies an infinite velocity of propagation.

The original Amperean electrodynamics, proceeding by consideration of elements of current, has not proved valid or sufficient in matters involving electric radiation, or even ordinary electrodynamic force. A most successful modification of it was that proposed by Weber, in which elements of current were replaced, as the fundamental object of consideration, by moving electric particles which acted on each other *at a distance* according to a law of force involving their velocities. This theory was, however, shown long ago by Lord Kelvin and Prof. von Helmholtz to be untenable on account of its violating the principles of the modern theory of energy; now, of course, direct action at a distance is altogether out of court.—Larmor (1896), ii. 616 f.

Opposition theories of electricity—the medium-theory of Maxwell

[13] The phrase 'and from which therefore the perpetual motion might be obtained' occurs in the first edition, but is suppressed in the second.

and Faraday, and the action-at-a-distance theories of Weber, Gauss, Neumann and others—are in the field against each other. Theories of physical phenomena, worked out on the hypothesis of direct forces across intervening space, . . . can only be provisional, and must necessarily be replaced by medium-theories as the science progresses.—Sir O. Lodge, PM 11 (1881) 530 f.

According to the theory that prevailed before Maxwell, electric and magnetic intensity were propagated with infinite velocity.—Houstoun, *Treatise on Light*, 1933, p. 398.

Weber carried out a magnificent summary, but one founded on the assumption of action-at-a-distance ; facts later showed that it did not correspond with reality.—P. Lenard, *Great Men of Science*, 1933, p. 339 note.

The doctrine of action-at-a-distance . . . was specially favoured by the French and German scientific schools, and in W. Weber's hands an almost complete electric theory was built upon it. The doctrine was, however, strongly repudiated by Newton himself, and hardly even became influential in the English school of abstract physicists.—Livens, ii. 51 f.

One recalls the mid-nineteenth-century theories of Weber, Riemann and Clausius, in which charged particles were fundamental. In all these theories, however, the particles were supposed to act on each other at a distance ; whereas in that of Lorentz the electrons interact with the medium in which they are embedded.—R. B. Lindsay and H. Margenau, *Foundations of Physics*, New York, 1936, p. 319.

Now we have already seen that the propagated potentials of Riemann and Lorenz were rejected by Maxwell. And Gauss, the initiator of the ballistic theory, explicitly based his view on non-instantaneous transmission. Writing in 1868, Clausius (x. 606) declares that the idea of Gauss, as expressed in his letter of 1845, gave rise to the efforts of Riemann, Neumann and Betti. 'All three authors,' he says, 'in different ways arrived at the result that, from the assumption that a time-interval is necessary for the transmission of electrical effects, the forces between two currents can be explained.' Writing in 1869, Stefan says (i. 694) :

> In recent times many attempts have been made—by Riemann, Loschmidt, Betti, C. Neumann—to find a theoretical basis for the connection between electrostatics and electrodynamics, which was begun by Weber's law concerning the mutual action of moving electricities, with the help of the thought suggested by Gauss that electrical actions are propagated with definite velocities.

It was in 1857 that Kirchhoff (i. 131), by what Brillouin (p. 226) calls 'adventurous simplifications,' showed that 'the velocity of propagation of an electric wave is independent of the section and

conductivity of the wire . . . and is very nearly equal to the velocity in empty space.' The idea of propagation was perfectly familiar at the time,[14] as is seen by the measurement of Kohlrausch and Weber in 1856, and the pronouncements of Weber (1846), Riemann (1858), Carl Neumann (1868), L. Lorenz (1867). In the midst of this movement, in 1862, Maxwell (iii. 500) concludes, by reasoning which is vitiated by algebraic slips, that 'we can scarcely avoid the inference that light consists in the transverse undulation of the same medium which is the cause of electric and magnetic phenomena.' The quasi-elastic undulation cannot be accepted to-day, whereas most of the principles of those who upheld 'electrical particles' are now honoured in physics.

The theory which is almost if not quite universally accepted to-day finds its fundamental synthetic expression in the Liénard force-law. Now, for all the experiments which Weber had in mind, it is sufficient to use the initial terms of this law as expanded in a Taylor series. It is this formula (7.17) which is now the orthodox successor of Weber's law (11.8). Prescinding from the acceleration-terms the essential difference is that Liénard's formula involves the velocities relative to the aether while that of Weber contains only the relative velocity of the point-charges. The inference that *therefore* Weber's formula involves instantaneous transmission, has now been completely refuted by Ritz, who showed that a law of Weber's type is itself the Taylor expansion of a force-formula based on the ballistic mode of transmission. So this objection also fails and Weber is again vindicated.

These considerations show the absurdity of the dogmatic pronouncement made by Heaviside (v. 504) :

> Germany was the breeding place and home of electrodynamic theories so-called. They never took root in England. Indeed Thomson and Tait severely condemned the method before Maxwell's treatise came out. Now Hertz squashed the electrodynamic theories visibly, and continental theorists were obliged to take up Maxwell.

This short review of the Weber controversy has been instructive. It demonstrates the futility of the premature closure of controversy in physics, it discloses the danger of authoritative orthodoxy in science. And it enables us to correct the grotesquely

[14] Maxwell (i. p. x) admits that both his method and 'the German one' 'have attempted to explain the propagation of light as an electromagnetic phenomenon and have actually calculated its velocity.'

inaccurate historical background which contemporary writers on electromagnetics assume. The following quotation from Lorentz is quite typical (xiv. 5):

> I must call your attention to the great and wonderful simplification which electrical theory has undergone in the course of the last half century. Formerly electrostatics, magnetism and electrodynamics were separate subjects, with but loose connections between them; and in the last-named there were many different theories like those of Wilhelm Weber, Gauss and Clausius, Ampère and Grassmann. Now all has been blended into one theory, the main equations of which can be written on a page of a pocket notebook. That we have got so far is due in the first place to Maxwell and next to him to Heaviside and Hertz.

And the Professor of Mathematical Physics in Yale gives the following summary in what he calls a 'historical introduction':

> The formulation in mathematical language of the discoveries of Coulomb, Ampère and Faraday, was undertaken by Maxwell, whose equations—slightly modified in form by Larmor and Lorentz—have been confirmed by every test which experiment offers.—Leigh Page, x. 216.

If any one man deserves credit for the synthetic idea which unifies the various branches of magnetic and electrical science, that man is Wilhelm Weber. To-day even those who uphold the aether-theory or profess to be relativists accept these principles introduced or developed by him: that Ampère's idea of magnetism as due to micro-currents can account for the relevant phenomena; that electricity has an atomic structure; that currents are streams of electrical particles; that Ampère's forces act directly between these particles and not between the conductors; that Coulomb's law must be modified for charges in motion; that, as Gauss said, action is not instantaneous; that the laws of electrodynamics and induction must be deduced, by statistical summation, from a force-formula for electrical particles. Even his ballistic principle, submerged for so long by aetherists and relativists, seems likely to challenge physicists once more in the developed form given to it by Walther Ritz.[15]

[15] The only credit given to Weber appears to be for his system of measurement. 'The introduction, by W. Weber, of a system of absolute units for the measurement of electrical quantities is one of the most important steps in the progress of the science.'—Maxwell, ii. 193. 'That name probably conveys but little meaning to the students of the present day, yet two generations ago he was a leader of science and one of the founders of our present system of electric units and measurement.'—Schuster, ii. 15.

5. The Scattering of Alpha Particles.

This subject will be briefly discussed here, because the results are nowadays extensively cited as a proof of the aether-electron theory. We begin with a short bibliography, which makes no pretensions to be complete ; it is sufficient for our purpose.

(*a*) Rutherford : 'The Scattering of Alpha and Beta Particles by Matter and the Structure of the Atom.'—PM 21 (1911) 669–688.

(*b*) Rutherford : *Radioactive Substances and their Radiations.* Cambridge, 1913.

(*c*) C. G. Darwin : 'Collision of Alpha Particles with Light Atoms.'—PM 27 (1914) 499–506.

(*d*) J. Chadwick and E. Bieler : 'The Collisions of Alpha Particles with Hydrogen Nuclei.'—PM 42 (1921) 923–940.

(*e*) Rutherford and Chadwick : 'Scattering of Alpha Particles by Atomic Nuclei and the Law of Force.'—PM 50 (1925) 889–913.

(*f*) Rutherford and Chadwick : 'The Scattering of Alpha Particles by Helium.'—PM 4 (1927) 605–620.

(*g*) Lord Rutherford, J. Chadwick and C. D. Ellis : *Radiations from Radioactive Substances.* Cambridge, 1930.

(*h*) M. A. Tuve, N. P. Heydenburg and L. R. Hafstad : 'The Scattering of Protons by Protons.'—PR 50 (1936) 806–825.

When α particles pass through matter, some of them are deviated from their original direction of motion. To account for the experimental results, Rutherford supposed that the positive charge associated with an atom is concentrated into a minute centre or nucleus.

> For the purposes of calculation he assumed that the central or nuclear charge of the atom and also the charge on the α particle behaved as point-charges. He was then able to show that for all deflections of the α particle greater than one degree the field due to the [surrounding] negative charge could be neglected, and the deflection due to the field of the central charge need alone be considered. The deflections due to the nuclear field can be calculated very simply if we assume that the electrical force between the nucleus and the α particle is given by Coulomb's law. We shall consider first the case of a heavy atom such that the nucleus may be assumed to remain at rest during the collision. The mass of the α particle will be taken as constant, since its velocity is always small in comparison to the velocity of light. The path of the α particle under these conditions will then be a hyperbola with the nucleus S of the atom as the external focus (*g*, p. 192).

This is illustrated in Fig. 52. The deflection is $\varphi = \pi - 2\alpha$. We have

$$OS = p/\sin\alpha, \quad OA = OB = OS\cos\alpha.$$

Hence

$$R = SA = SO + OA = p(1 + \cos\alpha)/\sin\alpha = p\cot\tfrac{1}{2}\alpha. \quad (11.15)$$

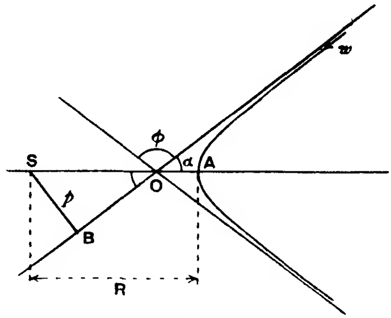

Fig. 52.

Let w be the velocity of the particle at a great distance and v its velocity when at A. Then the energy-equation is

$$\tfrac{1}{2}mw^2 = \tfrac{1}{2}mv^2 + ee'/R,$$

where e is the charge on the particle and e' that of the nucleus. That is

$$(v/w)^2 = 1 - 2k/R, \quad (11.16)$$

where $k = ee'/mw^2$.

The constancy of areal velocity (or the conservation of angular momentum) gives

$$vR = h = wp, \text{ or } v/w = p/R.$$

From these three equations we obtain

$$p = k\tan\alpha = k\cot\tfrac{1}{2}\varphi. \quad (11.17)$$

The scattering is determined by counting the number of par-

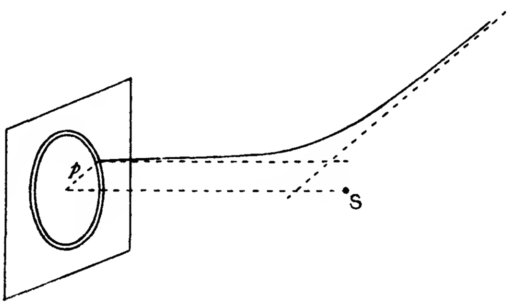

Fig. 53.

ticles falling normally on a constant area of a zinc sulphide screen at a constant distance from the radiator (Fig. 53). All the

particles passing through the ring $(p, p + dp)$ will be subject to a deviation between φ and $\varphi + d\varphi$. If N particles pass through unit area of the plane in one second, the number reaching the ring is $dn = N2\pi p dp$. These dn are uniformly distributed over a zone (area $2\pi \sin \varphi d\varphi$) of a unit sphere. Hence the number of particles deviated into a unit area of the unit sphere is from (11.17)

$$dn/2\pi \sin \varphi d\varphi = Nk^2/4 \, . \, \operatorname{cosec}^4 \tfrac{1}{2}\varphi.$$

That is, the number of particles scattered to unit area of the screen placed at an angle φ to the original direction of the particles is proportional to

$$(ee'/mw^2)^2/\sin^4 \tfrac{1}{2}\varphi. \qquad (11.18)$$

Experiment shows approximately that if w is kept constant the number of scintillations varies as $\operatorname{cosec}^4 \frac{1}{2}\varphi$, and if φ is kept constant the number varies as w^4. Rutherford's scattering formula is thus confirmed. If q is the electronic charge, $e = 2q$, $e' = Zq$, $m = 4m_H$; hence the formula can be also used for measuring the nuclear charge Zq.

From (11.15, 17) we see that the closest distance of approach is

$$R = k(1 + \sec \alpha),$$

where

$$k = Zq^2/mw^2 = 7 \, . \, 10^4 Z/w^2.$$

In the case of light elements the motion of the nucleus must be taken into account, and the treatment is more complicated.[16] In certain of such cases marked divergences from the scattering law have been found for very close distances of approach, i.e. for large velocities w. This anomalous scattering is attributed to failure of Coulomb's law. It is concluded that this law holds down to very small distances [17] :

Hydrogen	Helium	Aluminium
4·5	3·5	$4{\cdot}5 \times 10^{-13}$ cm.

Even for hydrogen ' the inverse square law holds at least approximately for the collisions of α particles of low velocity ' (d, p. 935).

We shall now make some critical comments on the foregoing conclusions. In the first place, the expression ' Coulomb's law ' is entirely out of place, for we are not dealing with electrostatics

[16] ' Even in the case of scattering by aluminium the correction is never greater than 4 per cent.' (g, p. 243).

[17] H. M. Taylor, PRS 134A (1932) 103 ; 136A (1932) 605. J. Chadwick, PRS 136A (1932) 745.

at all. It is Liénard's formula that is being tested. If in the approximate formula (7.17) we neglect the terms containing v' and f', we obtain

$$F_x = ee'r^{-2} \cos (rx).$$

But if we do not neglect the velocity terms, we have

$$F_x/ee'r^{-2} = \cos (rx)(1 + v'^2/2c^2 - 3v_r'^2/c^2 - \Sigma v_x v_x'/c^2) + v_x' v_r/c^2.$$

Hence the accepted treatment for scattering by light atoms is incorrect, for it professes to take v' into account and yet takes the force as that of the inverse square. So unconscious are the upholders of the prevalent electromagnetic theory of the fact that they are logically committed to Liénard's formula, that this contradiction has hitherto passed unperceived. Instead therefore of concluding that the statical force-law 'fails' at high velocities v', we can only state that the very assumption of the formula ee'/r^2 implies the neglect of $(v'/c)^2$. Moreover, on the prevalent theory, the force on the nucleus (e') is

$$F_x'/ee'r^{-2} = -\cos (rx)(1 + v^2/2c^2 - 3v_r^2/c^2 - \Sigma v_x v_x'/c^2) - v_x' v_r/c^2,$$

where we still draw r from e' to e. That is, $\mathbf{F}'$ is not equal and opposite to $\mathbf{F}$. Accordingly the assumption of the conservation of momentum for the colliding charged particles is inconsistent with the theory professedly held by these writers. Finally we would point out that, inasmuch as both velocity and rest are estimated relatively to the laboratory, the theory of an earth-convected aether is assumed in the foregoing treatment.

The next issue to be discussed is whether these experiments in any way decide against Ritz's force-law. Let us examine Rutherford's case in which m' may be taken to be approximately unmoved and v^2/c^2 is small. Neglecting the acceleration terms in Ritz's formula (11.7), putting $u = v$ and $F_x = m\ddot{x}$, we may express it thus:

$$\ddot{x} = \mu x/r^3 \, . \, (1 + 2Bv^2/c^2 - 3C\dot{r}^2/c^2) - 2\, D\mu\dot{x}\dot{r}/r^2, \quad (11.19)$$

where

$$\mu = ee'/m, \quad B = (3 - \lambda)/8, \quad C = (1 - \lambda)/4, \quad D = (1 + \lambda)/4. \quad (11.20)$$

In order to integrate this, we add to the right-hand side, P and Q being arbitrary,

$$P\mu/c^2 r \, . \, (\ddot{x} - \mu x/r^3) + Q\mu x/c^2 r^3 \, . \, (x\ddot{x} + yy - \mu/r).$$

This is legitimate, since these terms are of the order v^4/c^4. Hence, since $x\ddot{x} + y\ddot{y} = \dot{r}^2 + r\ddot{r} - v^2$,

$$\begin{aligned}\ddot{x} = \mu x/r^3 - (P+Q)\mu^2 x/c^2r^4 - (Q-2B)\mu x v^2/c^2r^3 \\ - (3C-Q)\mu x\dot{r}^2/c^2r^3 + P\mu\ddot{x}/c^2r + Q\mu x\ddot{r}/c^2r^2 - 2D\mu\dot{x}\dot{r}/c^2r^2.\end{aligned}$$

Similarly we can treat $\ddot{y}$. We thus have

$$\begin{aligned}\dot{x}\ddot{x} + \dot{y}\ddot{y} = \mu/r^3 \,.\, (x\dot{x} + y\dot{y}) - (P+Q)\mu^2/c^2r^4 \,.\, (x\dot{x} + y\dot{y}) \\ - (Q-2B)\mu v^2/c^2r^3 \,.\, (x\dot{x} + y\dot{y}) \\ - (3C-Q)\mu\dot{r}^2/c^2r^3 \,.\, (x\dot{x} + y\dot{y}) \\ + P\mu/c^2r \,.\, (\dot{x}\ddot{x} + \dot{y}\ddot{y}) + Q\mu\ddot{r}/c^2r^2 \,.\, (x\dot{x} + y\dot{y}) \\ - 2D\mu\dot{r}/c^2r^2 \,.\, (\dot{x}^2 + \dot{y}^2).\end{aligned}$$

Now

$$\dot{x}^2 + \dot{y}^2 = v^2, \quad x\dot{x} + y\dot{y} = r\dot{r}, \quad \dot{x}\ddot{x} + \dot{y}\ddot{y} = v\dot{v}.$$

Therefore

$$\begin{aligned}v\dot{v} - P\mu/c^2r \,.\, v\dot{v} - \mu/r^2 \,.\, \dot{r} + (P+Q)\mu^2/c^2r^3 \,.\, \dot{r} \\ + (Q - 2B + 2D)\mu v^2/c^2r^2 \,.\, \dot{r} + (3C-Q)\mu\dot{r}^3/c^2r^2 \\ - Q\mu/c^2r \,.\, \dot{r}\ddot{r} = 0. \qquad (11.21)\end{aligned}$$

Next consider the equation

$$\begin{aligned}\tfrac{1}{2}v^2 + A_1\mu/r \,.\, (1 - v^2/2c^2) + A_2\mu/r \,.\, (1 - \dot{r}^2/2c^2) \\ - A_3\mu^2/c^2r^2 = \text{constant}. \qquad (11.22)\end{aligned}$$

Differentiating with respect to the time, we have

$$\begin{aligned}v\dot{v} - A_1\mu/c^2r \,.\, v\dot{v} - (A_1 + A_2)\mu/r^2 \,.\, \dot{r} + 2A_3\mu^2/c^2r^3 \,.\, \dot{r} \\ + A_1\mu v^2/2c^2r^2 \,.\, \dot{r} + A_2\mu\dot{r}^3/2c^2r^2 \,.\, \dot{r}^3 \\ - A_2\mu/c^2r \,.\, \dot{r}\ddot{r} = 0. \qquad (11.23)\end{aligned}$$

Identifying equation (11.23) with (11.21), we equate the coefficients as follows :

$$A_1 = P, \quad A_1 + A_2 = 1, \quad 2A_3 = P + Q. \quad A_1 = 2Q - 4B + 4D,$$
$$A_2 = 6C - 2Q, \quad A_2 = Q.$$

That is,

$$\begin{aligned}A_1 &= 4C - 4B + 4D = (1+\lambda)/2, \\ A_2 &= 2C = (1-\lambda)/2, \\ A_3 &= -2B + 3C + 2D = 1/2.\end{aligned}$$

Reverting to equation (11.22), we see that in Ritz's theory 'the equation of energy,' replacing (11.16), is

$$\begin{aligned}\tfrac{1}{2}v^2 + (1+\lambda)\mu/2r \,.\, (1 - v^2/2c^2) + (1-\lambda)\mu/2r \,.\, (1 - \dot{r}^2/2c^2) \\ - \mu^2/2c^2r^2 = \text{constant} \\ = \tfrac{1}{2}w^2. \qquad (11.24)\end{aligned}$$

Since the force equation (11.19) contains a transverse component, it is easily seen that the areal velocity is not constant. Instead we have

$$H = r^2\dot{\theta} = h(1 + 2D\mu u/c^2), \tag{11.25}$$

where $h = wp$ and u, as usual in orbital questions, stands for $1/r$. Using the suffix 1 to denote differentiation with respect to the angle θ, we have

$$\dot{r} = \dot{\theta}r_1 = -Hu,$$
$$\dot{\theta} = Hu^2,$$
$$r^2\dot{\theta}^2 = H^2u^2.$$

Applying these formulae to the energy-equation (11.24), we easily find

$$u_1^2 = [1 + (1 - \lambda)\mu u/2c^2][-au^2 - 2bu + d],$$

where, using $k = \mu/w^2 = ee'/mw^2$ as before,

$$a = 1 - (\lambda + 2)(\mu/ch)^2 = 1 - (\lambda + 2)k^2w^2/c^2p^2,$$
$$b = k/p^2 \,.\, [1 + (1 + \lambda)w^2/4c^2],$$
$$d = 1/p^2.$$

Hence

$$\theta = \int \frac{du}{u_1}$$
$$= \int \frac{du}{\sqrt{-au^2 - 2bu + d}} - \frac{1-\lambda}{4}\frac{\mu}{c^2}\int \frac{udu}{\sqrt{-au^2 - 2bu + d}}$$
$$= \frac{1-\lambda}{4}\frac{\mu}{c^2}\Big[-au^2 - 2bu + d\Big]^{\frac{1}{2}}$$
$$+ \frac{1}{a^{\frac{1}{2}}}\left[1 - \frac{b}{a}\frac{1-\lambda}{4}\frac{\mu}{c^2}\right]\text{arc sin}\frac{au + b}{\sqrt{b^2 + ad}}.$$

When $u = 0$,

$$\theta_1 = \frac{1-\lambda}{4}\frac{k}{p}\frac{w^2}{c^2} + \beta\left[1 + \frac{3(1+\lambda)}{4}\frac{k^2}{p^2}\frac{w^2}{c^2}\right],$$

where

$$\tan\beta = \frac{b}{\sqrt{ad}} = \frac{k}{p}\left[1 + \frac{1+\lambda}{4}\frac{w^2}{c^2} + \frac{2+\lambda}{2}\frac{w^2}{c^2}\frac{k^2}{p^2}\right]$$
$$= \text{say, } k/p \,.\, (1 + B'w^2/c^2).$$

When $u_1 = 0$, i.e. when the particle is at the vertex A (Fig. 52)

$$\theta_2 = \frac{1}{a^{\frac{1}{2}}}\left[1 - \frac{b}{a}\frac{1-\lambda}{4}\frac{\mu}{c^2}\right]\frac{\pi}{2}$$
$$= \left[1 + \frac{3(\lambda+1)}{4}\frac{k^2}{p^2}\frac{w^2}{c^2}\right]\frac{\pi}{2}$$
$$= \text{say, } (1 + A'w^2/c^2)\pi/2.$$

Hence

$$\begin{aligned}\alpha &= \theta_2 - \theta_1 \\ &= \psi(1 + Aw^2/c^2) - (1-\lambda)kw^2/4pc^2,\end{aligned} \tag{11.26}$$

where $\psi = \pi/2 - \beta$, so that

$$\cot \psi = k/p \, . \, (1 + B'w^2/c^2).$$

The deflection is

$$\varphi = \pi - 2\alpha.$$

Differentiating $\cot \psi$ and φ, remembering that both A' and B' contain p, we obtain

$$-k d\varphi = 2dp \cos^2 \psi(1 + C'w^2/c^2),$$

where

$$C' = A' + B' + [(1-\lambda)/8 - 2B' - 2\psi \cos \psi] \operatorname{cosec}^3 \psi.$$

Hence

$$\frac{dn}{2\pi \sin \varphi d\varphi} = \frac{2\pi p dp}{2\pi \sin \varphi d\varphi}$$
$$= \frac{k^2}{4} \cdot \frac{\sin \psi}{\cos^3 \psi \sin \frac{1}{2}\varphi \cos \frac{1}{2}\varphi} \cdot \left(1 + D'\frac{w^2}{c^2}\right), \tag{11.27}$$

where D′ is easily found. To a first approximation ($\psi = \alpha = \pi/2 - \varphi$), this is identical with Rutherford's formula (11.18). Formula (11.27) is much more complicated, but it introduces only second-order corrections. Our sole interest here in this formula, and in the rather lengthy treatment which led to it, is to query whether the experimental results on scattering disprove, as is tacitly implied, Ritz's force-formula. Their accuracy is not such as to justify such a conclusion. In any case, Ritz's result (11.27) is so complicated that it cannot be said ever to have been experimentally tested.

The correction introduced by adopting Ritz's law involves the factors w^2/c^2 and $k^2w^2/p^2c^2 = w^2/c^2 \, . \, \tan^2 \frac{1}{2}\varphi$. Neglecting these and assuming the nucleus unmoved, we obtain Rutherford's formula.

It cannot be said that this has been verified very accurately. The experiments cited as affording 'abundant proof' (g, p. 197) give for $N \sin^4 \frac{1}{2}\varphi$—$N$ being the number of scintillations and the velocity w being kept constant—the following figures: for silver 18·4 to 30·6, for gold 28·8 to 38·4. Keeping φ constant and varying w (calculated by Geiger's rule $w^3 = \text{constant} \times \text{range}$) from 1×10^9 to $0{\cdot}6 \times 10^9$, so that the greatest value of w^2/c^2 is 1/900, the relative values of Nw^4 varied from 25 to 28 for gold and silver (g, p. 201). All these results are compatible with Ritz's formula.

Turning now to the case of lighter elements (with nucleus of mass M), a correction is found (g, p. 243) by substituting

$$\operatorname{cosec}^4 \tfrac{1}{2}\varphi - 2(m/M)^2 + 1 - 3(m/M)^2/2 \,.\, \sin^2 \varphi$$

instead of $\operatorname{cosec}^4 \frac{1}{2}\varphi$. This is obtained by assuming the law of action-reaction (valid only on Ritz's theory) and by assuming the inverse-square law, i.e. neglecting w^2/c^2. It is now found that the formula does not give good results for large values of w. Which is precisely what we should expect on Ritz's theory.

The present position is illustrated by the following quotations:

> These proton-scattering experiments demonstrate the existence of a proton-proton interaction which is violently different from the Coulomb repulsion for distances of separation of the order of 10^{-13} cm. The measurements are quantitatively in agreement . . . with a simple phase shift of the spherically symmetrical de Broglie wave ('S wave') due to the collision or scattering, corresponding to a new attractive force overpowering the Coulomb repulsion (h, p. 824 f.).
>
> If there is no other interaction between a pair of protons but the Coulomb repulsion, the scattering cross-section is given by the Rutherford formula as modified by Mott to take account of the possibility of exchange of the two protons. . . . Actually, experiments of White and of Tuve, Heydenburg and Hafstad do not agree at all with this formula. . . . This proves that there is another force acting besides the Coulomb force.—Bethe and Bacher, *Reviews of Mod. Physics*, 8 (1936) 130.
>
> The scattering of alpha-particles of polonium has been observed in helium, hydrogen and deuterium over a region of different velocities and scattering angles. The scattering associated with Coulomb fields takes place only at ranges much lower than was previously thought. This indicates departure from the Coulomb law of force at distances greater than 10^{-12} cm. The ratio of observed to 'classical' scattering was found to be large for hydrogen and deuterium, but the results for helium were found generally to be lower than previous estimates.—G. Mohr and G. Pringle, PRS 160A (1937) 206.

The present section is not designed to query the necessity of some such treatment as is contained in the quantum theory.[18] It is merely maintained that the Rutherford formula is not the sole expression of 'classical' theory and that its employment does not serve to prove the force-formula of Liénard as against that of Ritz.

We are tempted to add here some cognate remarks[18a]; but we omit any detailed discussion or proof, as the subject is beyond our scope. It is easy to see the *possibility*, on Ritz's theory, of explaining gravitational attraction as residual statistical forces between groups of moving charges; no such possibility is available from the Lorentz-Liénard theory. These forces must be due to terms of a high order and the forces will be small relatively to the first-order forces familiar to us in electromagnetics. This suggests a modification of Newton's law of the type of equation (11.19), namely:

$$\ddot{x} = -\mu x/r^3 \,.\, (1 - 2A\mu/c^2r + 2Bv^2/c^2 - 3C\dot{r}^2/c^2) + 2D\mu\dot{x}\dot{r}/c^2r^2, \tag{11.28}$$

where $\mu = \gamma Mm/m = \gamma M$. There is of course no reason why we should choose Ritz's electrodynamic values for the coefficients (11.20 with $A = 0$). Proceeding in exactly the same way as we did for the scattering of alpha-particles, we find the expression for θ as in equation (11.26). We easily deduce

(1) The rotation of the perihelion in one revolution is

$$(-A + 2B + 2D)2\pi\gamma M/c^2a(1 - e^2).$$

(2) The deviation of light near the sun is, if we put $w = c$, given by

$$(1 + 2B - C)2\gamma M/c^2p.$$

These results call for some comments.

(*a*) The result for Mercury's perihelion was given by Ritz (p. 421)—with the particular values $A = 0$, $B = (3 - \lambda)/8$,

[18] See N. F. Mott and H. S. W. Massey, *The Theory of Atomic Collisions*, Oxford, 1933; G. Breit, E. Condon, R. Present, 'Theory of Scattering of Protons by Protons.'—PR 50 (1936) 825–845.

[18a] A reader has queried the relevancy of these remarks. They are entirely irrelevant. But as the mathematical expressions involved have a close analogy with a certain commonsense treatment—not yet published—of general relativity, I thought it might be of interest to give here these suggestions whose significance will not escape any reader familiar with Einstein's theory of general relativity.

$D = (1 + \lambda)/4$—in 1908 long before the theory of general relativity was heard of.

(*b*) Einstein's theory, which delights every aesthetically minded mathematician, is a much less grandiose affair as judged and assessed by the physicist. It is nothing more and nothing less than using formula (11.28) with $A = B = C = D = 1$; while de Sitter took $A = 2$, $B = 1/2$, $C = 0$, $D = 2$.

(*c*) As regards the deviation of starlight near the sun, Einstein's (and de Sitter's) theory gives $1 + 2B - C = 2$. As is well known, this gives a deviation of 1·75 seconds of arc for a ray grazing the surface of the sun. But the Freundlich [19] eclipse expedition gives a deflection of 2·2.

(*d*) It may be objected that we are treating a light-ray like a particle. But that is exactly what general relativity does.

> It is fundamental in our theory that we can treat a ray of light like a moving particle.—N. Campbell, iv. 99.
>
> The curvature of the path of a light-ray passing near the sun signifies in fact nothing different from the curvature to which a point-mass, thrown into a gravitational field is subject.—H. Reichenbach, *Atom and Cosmos*, 1932, p. 142.
>
> The trajectory followed by a light-impulse is the same as that of a material particle moving with the velocity of light.—Painlevé-Platrier, *Cours de mécanique*, 1929, p. 618.

[19] 'There appears to be no further doubt possible that our series of measurements is not compatible with the value 1·75 asserted by theory.'—Freundlich, Klüber and Brunn, *Z. f. Astrophysik*, 3 (1931) 187. Cf. A. v. Brunn and H. v. Klüber, *ibid.* 14 (1937) 242.

CHAPTER XII

Moving Charges

1. Lorentz.[1]

Let us apply the Liénard formula (7.17) to find the action of a neutral current or magnet, moving with $\mathbf{v}'$, on a charge $+1$ moving with $\mathbf{v}$. At ds' we have q' moving with $\mathbf{v}'$ and $-q'$ with $\mathbf{v}' - w'$. Hence

$$\begin{aligned}
&\Sigma ee' = 0\\
&\Sigma ee'v'^2 = q'w'[2v' \cos(v'ds') - w']\\
&\Sigma ee'v_r'^2 = q'w' \cos(rds')[2v_r' - w' \cos(rds')]\\
&\Sigma ee'(\Sigma v_x v_x') = q'w'v \cos(vds')\\
&\Sigma ee'v_x' v_r = q'w_x'v_r = q'w'v \cos(vr) \cos(xds').
\end{aligned}$$

Inserting these in the formula, putting $q'w'/c = J'ds'$ and integrating over the circuit, we find that the x-component of the force on the unit charge comprises the following three portions :

$$(1)\ X_1 = v/c \,.\int ds'J'r^{-2}[\cos(vr)\cos(xds') - \cos(rx)\cos(vds')]$$

$$(2)\ X_2 = \int ds'J'v'/cr^2 \,.\, \cos(rx)[\cos(v'ds') - 3\cos(v'r)\cos(rds')]$$

$$(3)\ X_3 = \int ds'J'w'/2cr^2 \,.\, \cos(rx)[3\cos^2(rds') - 1]. \qquad (12.1)$$

Since w' is assumed to be negligible in comparison with v or v', we shall for the present neglect X_3.

[1] For brevity we call 'Lorentz' the theory which Lorentz should logically have given and which should be expounded by contemporary writers who profess to accept his views. As a matter of historical fact, Lorentz completely ignored the Liénard-Schwarzschild force-formula. Quite typical are the title of and the treatment in the article of Reiff and Sommerfeld (*Fernwirkungen, Actions à Distance*), whose historical survey—published in 1916—ends with Clausius! Needless to say, the function we call ψ is nowhere given.

If $\mathbf{A}$ is the vector potential, $A_x = \int J'dx'/r$. Hence

$$(\mathbf{v}\nabla)A_x = (\Sigma v_x \partial/\partial x)A_x = -\int v_r J'dx'/r^2$$

$$= -v\int J'ds'/r^2 \,.\, \cos\,(vr)\ \cos\,(xds').$$

And

$$\partial/\partial x \,.\, (\mathbf{vA}) = -v\int ds'J'/r^2 \,.\, \cos\,(rx)\cos\,(vds').$$

Therefore

$$X_1 = -c^{-1}(\mathbf{v}\nabla)A_x + c^{-1}\partial/\partial x \,.\, (\mathbf{vA}),$$

or by (1.2a)

$$\mathbf{R}_1 = c^{-1}V\mathbf{v}\ \mathrm{curl}\ \mathbf{A} = c^{-1}V\mathbf{vH}. \qquad (12.2)$$

This is the force exerted by a stationary circuit ($v' = 0$) on a moving charge (slow-moving cathode or canal rays, Zeeman and Hall effects, magnetic rotation of polarisation-plane).

We shall next evaluate X_2, taking J' to be uniform.

Since

$$\frac{\cos\,(rds')}{r^2} = \frac{\partial}{\partial s'}\frac{1}{r},$$

we have

$$3v'\cos\,(rx)\cos\,(v'r)\cos\,(rds')/r^2$$

$$= (x-x')\Sigma(x-x')v_x'\frac{\partial}{\partial s'}\frac{1}{r^3}$$

$$= \frac{\partial}{\partial s'}\left[\frac{x-x'}{r^3}\Sigma(x-x')v_x'\right] - \frac{1}{r^3}\frac{\partial}{\partial s'}\,[(x-x')\Sigma(x-x')v_x']$$

$$= \frac{\partial}{\partial s'}[\quad] + \frac{1}{r^3}\frac{\partial x'}{\partial s'}\Sigma(x-x')v_x' + \frac{x-x'}{r^3}\Sigma\frac{\partial x'}{\partial s'}\,v_x' - \frac{x-x'}{r^3}\Sigma(x-x')\frac{\partial v_x'}{\partial s'}$$

$$= \frac{\partial}{\partial s'}[\quad] + \frac{v'}{r^2}\cos\,(xds')\cos\,(v'r) + \frac{v'}{r^2}\cos\,(rx)\cos\,(v'ds') - \frac{g_r'}{r}\cos\,(rx),$$

where $\mathbf{g}' = \partial\mathbf{v}'/\partial s'$ so that $g_x' = \partial v_x'/\partial s'$.

Inserting this result in the integral for X_2, we find

$$X_2 = -c^{-1}J'\int ds'v'/r^2 \,.\, \cos\,(xds')\cos\,(v'r) - c^{-1}J'\int ds'g_r'/r \,.\, \cos\,(rx) \qquad (12.3)$$

If v' is a constant velocity of translation, clearly

$$X_2 = c^{-1}(\mathbf{v}'\nabla)A_x,$$

so that the total force is

$$\mathbf{R} = \mathbf{R}_1 + \mathbf{R}_2 = c^{-1}\nabla(\mathbf{vA}) - c^{-1}(\mathbf{v}\nabla)\mathbf{A} + c^{-1}(\mathbf{v}'\nabla)\mathbf{A}. \quad (12.4)$$

Hence if the circuit and the charge are moving with a common velocity of translation ($v' = v$),

$$\mathbf{R} = c^{-1}\nabla(\mathbf{vA}) \equiv \nabla\chi. \quad (12.5)$$

If the circuit is moving with constant v' and the charge is stationary ($v = 0$), the force on the charge is

$$\mathbf{R} = c^{-1}(\mathbf{v}'\nabla)\mathbf{A}. \quad (12.6)$$

We shall now express the force in a more convenient form. First taking Clausius's formula (7.19), we have

$$\begin{aligned} \delta L &= \Sigma ee'/r \,.\, (1 - \Sigma v_x v_x'/c^2) \\ &= -\,q'/c^2 r \,.\, \Sigma v_x w_x' \\ &= -\,c^{-1}J'ds'/r \,.\, \Sigma v_x \partial x'/\partial s' \\ &= -\,c^{-1}J'\Sigma v_x dx'/r. \end{aligned}$$

Hence

$$L = -\,c^{-1}\Sigma v_x A_x = -\,\chi, \quad (12.7)$$

where χ is $(\mathbf{vA})/c$.
And

$$\begin{aligned} R_x &= -\frac{\partial L}{\partial x} + \frac{d}{dt}\frac{\partial L}{\partial v_x} \\ &= \frac{1}{c}\left(v_x\frac{\partial A_x}{\partial x} + v_y\frac{\partial A_y}{\partial x} + v_z\frac{\partial A_z}{\partial x}\right) - \frac{1}{c}\frac{dA_x}{dt}, \end{aligned}$$

or

$$\mathbf{R} = \nabla_A\chi - \frac{1}{c}\frac{d\mathbf{A}}{dt},$$

where the suffix indicates that ∇ operates only on $\mathbf{A}$.

Hence, using $d/dt = \partial/\partial t + (\mathbf{v}\nabla)$, formulae (1.2) and (1.2a), we have on the theory of Clausius :

$$\begin{aligned} \mathbf{R} &= \nabla_A\chi - c^{-1}\mathbf{dA}/dt \\ &= \nabla\chi + c^{-1}V\boldsymbol{\omega}\mathbf{A} - c^{-1}\mathbf{dA}/dt \\ &= c^{-1}V\mathbf{vH} - c^{-1}\partial\mathbf{A}/\partial t. \end{aligned} \quad (12.8)$$

Turning now to Lorentz's formula for L (7.18), we have similarly

$$\delta L = - J'ds'/cr \, . \left[\Sigma v_x \frac{\partial x'}{\partial s'} + v'_r \cos(rds') - v' \cos(v'ds') - \tfrac{1}{2} w' \sin^2(rds') \right]$$

We omit the last term, w' being relatively negligible. Integrating over the circuit, we obtain

$$L = - (\chi + \psi), \tag{12.8a}$$

where

$$\psi \equiv c^{-1} \int ds' J' v'/r \, . \, [\cos(rds') \cos(v'r) - \cos(v'ds')]. \tag{12.9}$$

To evaluate this, taking J' to be uniform, we have

$$\begin{aligned} &rv' \cos(v'r) \cos(rds') \, . \, /r^2 \\ &= \Sigma(x - x')v'_x \frac{\partial}{\partial s'} \frac{1}{r} \\ &= \frac{\partial}{\partial s'} \Sigma \frac{(x - x')v'_x}{r} - \frac{1}{r} \frac{\partial}{\partial s'} \Sigma(x - x')v'_x \\ &= \frac{\partial}{\partial s'}(\qquad) + \frac{v'}{r} \cos(v'ds') - g'_r. \end{aligned}$$

Hence

$$\psi = - c^{-1} J' \int g'_r ds'. \tag{12.10}$$

So, on Lorentz's theory, the force is

$$\begin{aligned} \mathbf{R} &= \nabla_A(\chi + \psi) - c^{-1} \mathbf{dA}/dt \\ &= \nabla(\chi + \psi) + c^{-1} V \boldsymbol{\omega} \mathbf{A} - c^{-1} \mathbf{dA}/dt \\ &= \nabla\psi + c^{-1} V \mathbf{vH} - c^{-1} \partial \mathbf{A}/\partial t \end{aligned} \tag{12.11}$$

Whereas, according to Clausius, $\psi = 0$ and also there is no term in w' which is neglected.

2. Ritz.

Let us now similarly apply the Ritz formula (11.7) to find the action of a neutral current or magnet, moving with $\mathbf{v}'$, on a charge $e = +1$ moving with $\mathbf{v}$. The relative velocity of e and $+q'$

is $u_x = v_x - v'_x, \ldots$; that of e and $-q'$ is $v_x + w'_x - v'_x, \ldots$ Hence

$$\Sigma ee' = 0.$$

$$\begin{aligned}\Sigma ee'u^2 &= q'[(v_x - v'_x)^2 - (v_x + w'_x - v'_x)^2 + \ldots] \\ &= -q'[2v_x w'_x - 2w'_x v'_x + w'^2_x + \ldots] \\ &= -q'[2\Sigma u_x w'_x + w'^2] \\ &= -q'w'[2u \cos(uds') + w'].\end{aligned}$$

$$\begin{aligned}\Sigma ee'u_r^2 &= -q'(2v_r w'_r - 2w'_r v'_r + w'^2_r) \\ &= -q'(2u_r w'_r + w'^2_r) \\ &= -q'w'[2u \cos(ru)\cos(rds') + w' \cos^2(rds')].\end{aligned}$$

$$\begin{aligned}\Sigma ee'u_x u_r &= q'[(v_x - v'_x)(v_r - v'_r) - (v_x + w'_x - v'_x)(v_r + w'_r - v'_r)] \\ &= -q'[v_x w'_r + v_r w'_x + w'_r w'_x - w'_r v'_x - w'_x v'_r] \\ &= -q'[u_x w'_r + u_r w'_x - w'_r w'_x] \\ &= -qw'[u \cos(ux)\cos(rds') + u \cos(rds')\cos(xds') \\ &\qquad - w' \cos(rds')\cos(xds')].\end{aligned}$$

Inserting these results in the formula, putting $q'w'/c = J'ds'$, and integrating over the circuit, we find that the x-component of the force comprises the three parts :

(1) and (2)

$$\begin{aligned}X_1 + X_2 = -\int ds'J'u/cr^2 \,.\, \Big[\cos(rx)\Big\{\frac{3-\lambda}{2}\cos(uds') \\ -\frac{3(1-\lambda)}{2}\cos(rds')\cos(ru)\Big\} \\ -\frac{1+\lambda}{2}\{\cos(xu)\cos(rds') + \cos(ru)\cos(xds')\}\Big]\end{aligned}$$

(3) $$\begin{aligned}X_3 = -\int ds'J'w'/cr^2 \,.\, \Big[\cos(rx)\Big\{\frac{3-\lambda}{4} - \frac{3(1-\lambda)}{4}\cos^2(rds')\Big\} \\ -\frac{1+\lambda}{2}\cos(rds')\cos(xds')\Big] \qquad (12.12)\end{aligned}$$

Since w' is assumed to be negligible in comparison with u, we shall for the present neglect X_3.

Utilising the result proved on p. 547, we have

$$3u/r^2 \,.\, \cos(rx)\cos(rds')\cos(ru)$$

$$= \frac{\partial}{\partial s'}(\quad) + u/r^2 \,.\, \cos(xds')\cos(ur) + u/r^2 \,.\, \cos(rx)\cos(uds') + g'_r/r \,.\, \cos(rx),$$

since $\partial u_x/\partial s'_x = -\partial v'_x/\partial s'$. (12.12a)

Inserting this in the integral for $X_1 + X_2$, we find we can divide it into

$$X_1 = J' \int ds'u/cr^2 . [\cos (ur) \cos (xds') - \cos (rx) \cos (uds')]$$

and

$$X_2 = \frac{1+\lambda}{2}\frac{J'}{c}\int ds'u/r^2 . \cos (xu) \cos (rds') + \frac{1-\lambda}{2}\frac{J'}{c}\int ds'g_r'/r. \tag{12.13}$$

If $\mathbf{v}$ and $\mathbf{v}'$ are constant in magnitude and direction, X_2 is clearly zero and, as proved for formula (12.2),

$$\mathbf{F} = \mathbf{F}_1 = c^{-1}V\mathbf{uH}. \tag{12.14}$$

Hence when the circuit and the charge are moving with a common velocity of translation ($\mathbf{u} = 0$), $\mathbf{F} = 0$. (Observe that we use $\mathbf{F}$ for Ritz and $\mathbf{R}$ for Lorentz.)

An alternative way of dividing up the force $X_1 + X_2$, is provided by putting

$$u \cos (uds') = v \cos (vds') - v' \cos (v'ds')$$

etc. We then have :

(1) $X_1 = v/c \int ds'J'/r^2 . [\cos (vr) \cos (xds') - \cos (rx) \cos (vds')]$,

so that $\mathbf{F}_1 = c^{-1}V\mathbf{vH}$, as in the case of (12.2),

and

$$\begin{aligned}(2)\ X_2 = \int ds'J'v'/2cr^2 . \Big[&\cos (rx)\{(3-\lambda) \cos (v'ds') \\ &- 3(1-\lambda) \cos (rds') \cos (rv')\} \\ &- (1+\lambda)\{\cos (xv') \cos (rds') + \cos (rv') \cos (xds')\}\Big] \\ = - J'\int ds'v'/cr^2 . &[\cos (v'r) \cos (xds') - \cos (rx) \cos (v'ds')] \\ &- (1+\lambda)J'/2c . \int ds'v'/r^2 . \cos (v'x) \cos (rds') \\ &+ (1-\lambda)J'/2c . \int ds'g_r'/r. \end{aligned} \tag{12.15}$$

Hence, as in Lorentz's theory, the force exerted by a stationary circuit ($v' = 0$) on a moving charge is $c^{-1}V\mathbf{vH}$.

If the circuit is moving with constant $\mathbf{v}'$ and the charge is stationary ($v = 0$), the force on the charge is

$$X_2 = -J'v'/c \,.\int ds'/r^2 \,. [\cos(v'r)\cos(xds') - \cos(rx)\cos(v'ds')]$$

or

$$\mathbf{F}_2 = -c^{-1}V\mathbf{v}'\mathbf{H}. \qquad (12.16)$$

Turn now to Ritz's formula (11.13)

$$\delta L = \Sigma \frac{ee'}{r}\left[\frac{1-\lambda}{2}\left(1+\frac{u_r^2}{2c^2}\right)+\frac{1+\lambda}{2}\left(1+\frac{u^2}{2c^2}\right)\right].$$

We have

$$\Sigma ee' = 0.$$
$$\Sigma ee'u_r^2 = -q'w'[2v_r\cos(rds') - 2v_r'\cos(rds') + w'\cos^2(rds')]$$
$$\Sigma ee'u^2 = -q'w'[2v\cos(vds') - 2v'\cos(v'ds') + w'].$$

Hence δL is

$$-J'ds'/2cr\,.\Big[(1-\lambda)v_r\cos(rds') + (1+\lambda)v\cos(vds')$$
$$-(1-\lambda)v_r'\cos(rds') - (1+\lambda)v'\cos(v'ds')$$
$$+\tfrac{1}{2}w'\{(1-\lambda)+(1-\lambda)\cos^2(rds')\}\Big].$$

Neglecting the terms in w', consider the terms containing v. Since

$$\frac{v_r\cos(rds')}{r} = \Sigma v_x(x-x')\frac{\partial}{\partial s'}\frac{1}{r}$$
$$= \frac{\partial}{\partial s'}\Sigma\frac{x-x'}{r}v_x - \frac{1}{r}\frac{\partial}{\partial s'}\Sigma v_x(x-x')$$
$$= \frac{\partial}{\partial s'}(\qquad) + \frac{1}{r}\Sigma v_x\frac{\partial x'}{\partial s'},$$

the v terms in L give, when integrated,

$$-J'/c\,.\int\Sigma v_x dx'/r = -\chi. \qquad (12.16b)$$

And, as in treating Lorentz's formula, the v' terms give

$$-\psi \equiv J'2c\int ds'[(1-\lambda)/r\,.\,v'\cos(v'ds') - (1-\lambda)g_r'$$
$$+(1+\lambda)/r\,.\,v'\cos(v'ds')]$$
$$= J'/c\,.\int ds'[v'/r\,.\cos(v'ds') - (1-\lambda)/2\,.\,g_r']. \qquad (12.17)$$

Hence $L = -(\chi + \psi)$.

Therefore, neglecting the term $q\lambda f'_r/2c^2$ in the formula (11.14), we have

$$\begin{aligned} \mathbf{F} &= \nabla(\chi + \psi) + c^{-1}V\boldsymbol{\omega}\mathbf{A} - c^{-1}d\mathbf{A}/dt \\ &= \nabla\psi + c^{-1}V\mathbf{v}\mathbf{H} - c^{-1}\partial\mathbf{A}/\partial t. \end{aligned} \tag{12.18}$$

The formula is formally identical with that of Lorentz (12.11), but ψ has a different meaning. For example, if the system has a common constant velocity, then according to Lorentz $\psi = 0$ and $\mathbf{R}$ is not zero; but according to Ritz $\psi = -\chi$ and $\mathbf{F} = 0$.

In this formula we have neglected the term

$$\lambda/2c^2 \,.\, \nabla(qf'_r)$$

which occurs in (11.14). This gives a contribution (to ψ)

$$\frac{\lambda}{2c}\int \frac{dJ'}{dt}\, ds' \cos\,(rds').$$

If, as we have already done, we take J' to be uniform along s', this will be zero.

We can now compare our results with those of Chapter IV. We there obtained (4.79a): $L = \varphi - \chi$. But in the case of neutral currents, $\varphi = 0$; that is, $L = -\chi$, which results from the formula of Clausius (12.7). And (4.81) with $\lambda = 1$ gives

$$\mathbf{A} = \int J'\mathbf{ds}'/r,$$

which we also obtain from Liénard's formula. But on the Liénard-Lorentz theory we find (12.8a): $L = -(\chi + \psi)$. This also follows from Ritz's formula (12.17a), though ψ has a different meaning. Observe also that, both for Ritz and for Lorentz, we have (12.11, 18),

$$\mathbf{F} = \mathbf{F}_0 + c^{-1}V\mathbf{v}\mathbf{H}$$

where

$$\mathbf{F}_0 = \nabla\psi - c^{-1}\partial\mathbf{A}/\partial t.$$

Here ψ is *not* $-\varphi$ and $\mathbf{F}_0$ is *not* $\mathbf{E}$; in fact φ is zero and so is $\mathbf{E}$ (at least very approximately, for we are neglecting the component X_3). This point should be remembered when we come to sections 4 and 8 of this chapter. The reason for the emergence of the quantity ψ is that we have divided the motion of the moving charges into two parts: (1) the linear conductor and the positive ions move with $\mathbf{v}'$, (2) the negative ions move with $-\mathbf{w}'$ relatively to the conductor.

Confining ourselves to the v terms in δL (12.16a), we have on integration $L = -(\mathbf{vA})/c$, where

$$A_x = \int ds' \left[\frac{1+\lambda}{2r} J'_x + \frac{1-\lambda}{2r} J'_r \cos(rx) \right], \qquad (12.18a)$$

that is, formula (4.21) or (4.81). As we have just shown (12.16b), the λ terms give zero when the current is uniform. But on Ritz's theory, as distinct from those of Clausius and Liénard, there is no reason for putting $\lambda = 1$; for we obtain the force-formula (4.12 = 11.6b) with $k = \lambda$.

3. The Magnetic Field of a Moving Charge.

Having obtained an expression for the force exerted by a circuit or magnet on a moving charge, we now proceed to investigate the force exerted by a moving charge on a circuit or magnet. On Ritz's theory, of course, there is no fresh problem involved; for the two forces are equal and opposite. Let us see what, on Lorentz's theory, is the force exerted by e' moving with $\mathbf{v}'$ on $+q$ moving with $\mathbf{v}$ and on $-q$ moving with $\mathbf{v}-\mathbf{w}$.

$$\Sigma ee' = \Sigma ee'v'^2 = \Sigma ee'v_r'^2 = 0.$$

$$\begin{aligned}\Sigma ee'(v_x v'_x + \ldots) &= e'q(v_x v'_x - (v_x - w_x)v'_x \ldots) \\ &= e'qwv' \cos(v'ds).\end{aligned}$$

$$\Sigma ee'v'_x v_r = e'qwv'w \cos(v'x) \cos(rds).$$

Inserting these results in the Liénard formula (7.17), and putting $qw/c = Jds$, we find

$$dR_x = Je'v'ds/cr^2 \,.\, [\cos(v'x)\cos(rds) - \cos(rx)\cos(v'ds)].$$

Now

$$v'rds[\cos(v'x)\cos(rds) - \cos(rx)\cos(v'ds)]$$

is the x-component of $\mathbf{v}'(\mathbf{rds}) - \mathbf{r}(\mathbf{v}'\mathbf{ds})$, i.e. of $V\mathbf{ds}V\mathbf{v}'\mathbf{r}$. Hence

$$\mathbf{dR} = Je'/cr^3 \,.\, V\mathbf{ds}V\mathbf{v}'\mathbf{r}. \qquad (12.19)$$

But we have shown (4.5) that the force on $\mathbf{ds}$ due to a magnetic field is $JV\mathbf{dsH}$. Therefore the magnetic field of the moving charge is

$$\mathbf{H} = e'/cr^3 \,.\, V\mathbf{v}'\mathbf{r}, \qquad (12.20)$$

r being drawn from the charge e' to the point. This is the formula which is usually written

$$H = e'v' \sin\alpha \,.\, /r^2,$$

with e' in elms. Putting $\mathbf{E}' = e'\mathbf{r}_1/r^2$, (12.20) can also be written in the form

$$\mathbf{H} = c^{-1}V\mathbf{v}'\mathbf{E}'. \tag{12.20a}$$

It is easy to verify that these results hold also on the theory of Clausius.

Interchanging dashes and changing sign (so that r is now drawn from the current element), we have for the action of $e = +1$ on the circuit

$$R'_x = -v/c\,.\int ds'J'/r^2[\cos(vx)\cos(rds') - \cos(rx)\cos(vds')]$$

or, taking J' to be uniform,

$$\begin{aligned} R'_x &= -J'v_x/c\,.\int ds'\cos(rds')\,.\,/r^2 \\ &\quad + J'v/c\,.\int ds'/r^2\,.\cos(rx)\cos(vds') \\ &= -\frac{1}{c}\frac{\partial}{\partial x}(\mathbf{vA}), \end{aligned}$$

since the first integral is zero.

Hence

$$\mathbf{R}' = -\nabla\chi, \tag{12.21}$$

independently of the velocity (v') of the circuit.

But we have already found (12.4) that, when $\mathbf{v}'$ is a constant velocity of translation, the force exerted by the circuit on the unit charge is

$$\mathbf{R} = \nabla\chi - c^{-1}(\mathbf{v}\nabla)\mathbf{A} + c^{-1}(\mathbf{v}'\nabla)A. \tag{12.4}$$

Therefore $\mathbf{R}$ and $\mathbf{R}'$ are equal and opposite only when $\mathbf{v} = \mathbf{v}'$, i.e. when the circuit and the charge are moving with a common velocity of translation. When the circuit is stationary $(\mathbf{v}' = 0)$

$$\mathbf{R} + \mathbf{R}' = -c^{-1}(\mathbf{v}\nabla)\mathbf{A},$$

so that the law of action-reaction does not hold. It might be considered possible to test this experimentally. But in practice e would have to be taken moving over a closed path and the mean force per revolution measured. Since $\mathbf{v} = \mathbf{ds}/dt$

$$\frac{1}{T}\int(\mathbf{R} + \mathbf{R}')dt = -\frac{1}{cT}\int(\mathbf{ds}\nabla)\mathbf{A},$$

which is zero for a closed circuit.

Let us now turn to Ritz's theory. Putting $\mathbf{v}' = 0$, we have from (12.16)

$$F'_x = -F_x = -J'ev/c \, . \int ds'/r^2 \, . \, [\cos(vr)\cos(xds') \\ - \cos(rx)\cos(vds')]$$

Putting $ev/c = Jds$ and integrating over a complete circuit of the moving charge, we obtain for the forces

$$\mp JJ' \iint dsds'/r^2 \, . \, [\cos(xds')\cos(rds) - \cos(rx)\cos(dsds')].$$

Comparing this with (4.5), we see that the moving charge is equivalent to a current.

We have

$$\mathbf{R}' = -e\nabla\chi \qquad (12.5)$$
$$\mathbf{F}' = -e/c \, . \, V\mathbf{v} \text{ curl } \mathbf{A} \qquad (12.16)$$
$$= \mathbf{R}' + e/c \, . \, (\mathbf{v}\nabla)\mathbf{A}. \qquad (12.22)$$

Since, as we have just seen, the latter term gives zero over a closed path of the charge, the mean values of $\mathbf{F}'$ and $\mathbf{R}'$ are equal. Hence both theories give the same result.

We have therefore found, according to both theories, what is called the magnetic field of a moving charge. We have done so, relying only on accepted principles of the electron theory, without contradicting the Amperian view of magnetism. We have merely particularised the general idea of the force exerted by one moving charge on another by taking the force exerted by a moving singlet on an element of neutral current. Then, in accordance with the experiments of Rowland (1876) and others, we took the singlet as moving in a closed circuit. And we found that the experiments are equally explicable by any of the three versions—Clausius, Lorentz, Ritz—of the electron theory. Accordingly we reject as incompatible with professedly accepted views of electricity and magnetism such metaphorical descriptions as that given by Swann (x. 59):

> The moving charge is accompanied by lines of magnetic force. . . . These magnetic lines do nothing to another charge so long as that other charge is at rest. If it moves, however, they exert a force on it over and above that exerted by the electric field.

These 'lines' are merely a vivid non-mathematical representation of a vector-function. If we have only two charges, the relevant vector is $\mathbf{F}$, given to the second-order by (7.17) or (11.7).

There is no 'magnetic field' until we produce a 'magnet,' i.e. a set of moving charges constituting a closed neutral current. This is the only statement logically reconcilable with the premisses which everyone admits to-day.

Nowadays, when the electron theory is universally admitted, the phenomena described as the magnetic effects of a moving charge seem pretty obvious. And we are tempted to read our present outlook back into the heyday of Maxwellianism. Witness these typical quotations :

> The existence of convection currents was foreseen theoretically by Maxwell and was experimentally verified by Rowland and his pupils.—Rougier, *La matière et l'énergie*, 1921², p. 48.
>
> Rowland's experiments on electric convection are in agreement with Maxwell's teaching.—J. J. Thomson, vi. 237.
>
> The possibility that a moving electric charge might produce a magnetic field occurred to Faraday. . . . The effect was observed by Rowland in 1876 and again by Röntgen in 1885.—Jeans, p. 514.
>
> Since an electric current is simply the passage of electric charge, a charged conductor when moved should itself affect a magnet in its neighbourhood. Such an effect was observed by Rowland in 1876.—Pidduck, p. 106.

Rowland himself (p. 30), writing in 1878, was much more diffident and correctly represents the dominant outlook of the time :

> The experiments described in this paper were made with a view of determining whether or not an electrified body in motion produces magnetic effects. There seems to be no theoretical ground upon which we can settle the question, seeing that the magnetic action of a conducted electric current may be ascribed to some mutual action between the conductor and the current. Hence an experiment is of value.

This hesitation is hardly intelligible to us to-day ; we tend in fact to the other extreme, i.e. to claim for the experiments a result which is inferentially incompatible with Ritz's theory. This seems to be the claim made by Sir James Jeans :

> It may be objected that the foregoing experiments [of Rowland, Röntgen, Pender] only test the magnetic field produced by a continuous chain of electric charges moving in a closed circuit ; but this objection cannot be urged against experiments performed by E. P. Adams in 1901. In these experiments charged brass spheres were made to pass a suspended magnetic needle at the rate of about 800 per second, and the apparatus was arranged so that the effect

of one sphere had almost disappeared before the needle came under the influence of the next.—Jeans, p. 515.

In 1901 Prof. E. P. Adams attached a series of charged brass spheres to the circumference of a rotating wheel and found that these produced a magnetic field which alternated periodically as the spheres passed by a suspended magnetic needle.—Jeans, *Atomicity and Quanta*, 1926, p. 49.

The allusion is to a paper on 'The Electromagnetic Effects of Moving Charged Spheres,' by E. P. Adams, published in *Am. J. Sci.* 12 (1901) 155–167. A quotation from this article will show at once that Adams dealt merely with the mean effects due to charges moving in closed circuits.

An attempt will be made in the following to compare the results obtained with the results expected from theory. From reasons which will appear later this comparison can be regarded only as approximate, and is given merely to show that the observed results are of the right order of magnitude. . . . Figure 4 is plotted from this expression [$H = qv/r^2 . \sin\theta$], and shows how the force varies with the position of the spheres. The upper curve gives the resultant force at the lower needle due to both sets of spheres ; and the lower curve, which is nearly a straight line, gives the force at the upper needle. Let the mean value of the force at the lower needle, obtained by time-integration of the curve, be $2\pi NqA/c$, and the mean value of the force at the upper needle $2\pi NqB/c$. Then the effect on the needle will be the same as if constant forces of these magnitudes acted upon it.—Adams, pp. 160–162. Also in PM 2 (1901), 291–294.

Returning to formula (12.20a) of the aether-electron theory, we have

$$\mathbf{H} = c^{-1} V\mathbf{v}\mathbf{E} \qquad (12.20a)$$

as the magnetic field of a charge moving with $\mathbf{v}$. Suppose we have a *uniformly* moving charge, so that

$$0 = \mathbf{dE}/dt = \partial\mathbf{E}/\partial t + (\mathbf{v}\nabla)\mathbf{E}.$$

Then by (1.6)

$$\begin{aligned} \text{curl}\,\mathbf{H} &= c^{-1}[-(\mathbf{v}\nabla)\mathbf{E} + (\mathbf{E}\nabla)\mathbf{v} - \mathbf{E}\,\text{div}\,\mathbf{v} + \mathbf{v}\,\text{div}\,\mathbf{E}] \\ &= -c^{-1}(\mathbf{v}\nabla)\mathbf{E} \\ &= -\dot{\mathbf{E}}/c, \end{aligned} \qquad (12.23)$$

since the last three terms in the squared bracket are zero (div $\mathbf{E} = 0$ outside e). This last equation, which holds (to the second-order) *only* for a uniformly moving charge, has been erroneously identified with Maxwell's equation for his displacement-current.

The impossibility of this is at once evident from the facts that (1) the displacement-current is merely a mathematical way of referring to the propagated potentials, (2) the formula (12.23) follows from Clausius's theory which neglects propagation. We must therefore reject the alleged proof of (12.20a) by deduction from (12.23), as expounded in such assertions as the following [1a] :

> A simple but important example of the use of Maxwell's equations : . . . The experiments not only prove the existence of the magnetic field produced by moving charges but also confirm Maxwell's theory quantitatively.—Jeans, pp. 513, 515.
>
> This prediction [formula (12.20)] received a complete experimental quantitative confirmation in the experiments on the convection-current. This latter appears to us as a necessary consequence of Maxwell's displacement-current. . . . These experiments must be considered as verifying . . . the accuracy of the law of the displacement-current.—Langevin, ii. 71.
>
> We may logically argue that if the displacement current is sufficiently real to enable us to complete the condenser circuit, it must be able to produce a magnetic field. This has been proved to be true in the experiments of Rowland.—F. White, p. 21.
>
> Since by Maxwell's theory only closed currents can exist, it was a necessary consequence of the development of this theory that every moving body, if it be electrically charged, must produce a magnetic field exactly as does a conduction current. . . . This conclusion was experimentally verified in 1876 as a result of an important research carried out by Rowland.—Haas, i. 239.

The answer to this argument has now been given. The experiments verified the law for H only for a closed path. For such the theory of Ritz gives the same result as that of Maxwell-Lorentz, with the added advantage that it does not violate the principle of the equality of action and reaction. Moreover, according to Lorentz, the velocity which occurs in assimilating the path of the charge to a current-circuit ($ev/c = Jds$), is the absolute velocity through the stagnant aether; whereas the experiments verified the result, taking v to be the velocity of the charge relative to the laboratory in which the other circuit (magnet) is at rest. The experiments may therefore be taken as agreeing either (1) with the theory of an earth-convected aether or (2) with the ballistic theory.

Since the above was written I have come across a stimulating text-book from which I propose to quote a passage for criticism :

[1a] This alleged proof is also to be found in other text-books, e.g. N. Campbell, iii. 23, Becker, p. 39 f.

> From theoretical considerations which are basic to the theory of relativity, but which are unfortunately beyond the scope of this book, it can be shown that a consequence of the fact that electrical disturbances do not travel instantaneously is that the repulsive force e^2/r^2, which two charged bodies exert upon one another electrostatically, diminishes—as measured by an observer at rest—when they are set in motion along parallel paths. . . . This diminution of force is attributed to the appearance of 'magnetic' force, which partly neutralises the electrostatic force. [When v/c is small] the force e^2/r^2 which they exert upon one another at rest becomes diminished by an amount $(v^2/c^2)e^2/r^2$, where v is the velocity of the charges relative to the observer and c is the velocity with which the electric disturbance travels. This diminution constitutes the magnetic force. . . . The appearance of magnetic force may be very simply and strikingly demonstrated by loosely fastening two parallel wires several metres long and a few centimetres apart on an insulating frame and passing a current of a few ampères through them. If the direction of the flow of current is the same in the two wires, they attract one another.—Pilley, p. 186 f.

There is here a false implication, namely, that the magnetic force exerted by a moving charge cannot be investigated except with the help of Einstein's theory. That theory professes to establish a concatenation between the observations of different observers, and it claims to do this by 'transforming' the measures obtained by the laboratory-physicist. How then can the theory be used to explain its own data or premisses? The author's explanation is merely an elementary, and inaccurate, deduction from Liénard's force-formula. The references to 'the observer' are merely a concession to the prevalent jargon; he really means the laboratory, relatively to which the velocity v is measured. In formula (7.17) take $v' = v$ along the x-axis. Then, neglecting acceleration,

$$F_x = ee'r^{-2} \cos (rx) \,.\, [1 + v^2/2c^2 - 3v^2/2c^2 \,.\, \cos^2 (rx)],$$
$$F_y = ee'r^{-2} \cos (ry) \, [1 - v^2/2c^2 - 3v^2/2c^2 \,.\, \cos^2 (rx)].$$

When the charges were at rest in the laboratory, the force between them was ee'/r^2. It is not true to say that this becomes diminished in the ratio $1 - v^2/c^2$. It is not correct to call this diminution a magnetic force. And it is wrong to assert that this modification of the force is a consequence of the fact that electrical disturbances do not travel instantaneously. Finally, we cannot apply this to deduce the Amperian inter-circuit formulae unless we assume neutral circuits. So we must reject this offhand pseudo-relativist simplification.

4. The Force on a Moving Charge.

The formula (4.31b) [2]

$$\mathbf{F} = e(\mathbf{E} + c^{-1} V\mathbf{vH})$$

has, of course, been assumed in deriving the Liénard force-formula. Let us now reverse the argument; assuming Liénard's formula as the synthesis of electromagnetic experiments, let us investigate the force on a moving charge. The ordinary proof of the formula is extremely unsatisfactory. Here is Lorentz's effort:

> It is got by generalising the results of electromagnetic experiments. The first term represents the force acting on an electron in an electrostatic field [$\mathbf{F}_1 = e\mathbf{E}$]. . . . On the other hand, the part of the force expressed by the second term may be derived from the law according to which an element of a wire carrying a current is acted on by a magnetic field [$\mathbf{F}_2 = j/c \,.\, V\mathbf{dsH}$]. . . . Now, simplifying the question by the assumption of only one kind of moving electrons with equal charges and a common velocity, we may write [$j\mathbf{ds} = e\mathbf{v}$]. . . . After having been led in one particular case to the existence of the force [$\mathbf{F}_1 = e\mathbf{E}$] and in another to that of the force [$\mathbf{F}_2 = e/c \,.\, V\mathbf{vH}$], we now combine the two in the way shown in the equation, going beyond the direct result of experiments by the assumption that in general the two forces exist at the same time.—Lorentz, viii. 14 f.

There are two overwhelming objections to this alleged generalisation. (1) The two 'particular cases' here 'combined' are quite incompatible. In the one case we have charges at rest, in the other the charges are moving; they cannot be both stationary and moving. (2) Experiments with 'a wire carrying a current' have to do with *neutral* currents, yet the derivation contradicts this neutrality.[3]

[2] Van Vleck calls it 'the fundamental force-equation postulated by the electron theory' (p. 17). But such a title should clearly be reserved for the alternative force-formulae of Ritz and Liénard.

[3] The current text-books are equally unsatisfactory. Slater and Frank, p. 240: 'The electrical force per unit volume is $\rho\mathbf{E}$. . . . The magnetic force is that acting on the current. . . . Thus we have for the force-vector $\mathbf{F} = \rho\mathbf{E} + c^{-1} V\mathbf{uH}$. If the current density is produced by the motion of charge [i.e. the same charge as gives the term $\rho\mathbf{E}$], we have $\mathbf{u} = \rho\,\mathbf{v}$.' Försterling, p. 58: 'While the force on a resting charge depends only on **E**, the force on a moving charge is increased by a term corresponding to the law of Biot-Savart'—which was proved only for a closed uniform neutral current. Schaefer (i. 673): 'The first part is simply the electrostatic force corresponding to Coulomb's law. . . . The second part is the force exerted by a magnetic field **H** on a moving charge; it corresponds to that force of the old theory which acts on a current-carrying conductor according to the Biot-Savart law.' Planck (p. 242) obtains the formula 'by combining,' Fürth (p. 341) by 'addition.'

In order to see that there is a concealed assumption involved in the argument, let us turn to Ritz's formula which, as we have seen, gives $e\mathbf{E}$ for the electrostatic case and also $j/c\,.\,V d\mathbf{s}\mathbf{H}$ for electromagnetic experiments with linear metallic circuits. Ritz's formula satisfies each of the two premisses—does it satisfy the conclusion ? In formula (11.7), which gives the force of e' moving with $\mathbf{v}'$ on e moving with $\mathbf{v}$, neglect v'^2/c^2 and the acceleration terms. We find $F_x = X_1 + X_2$, where

$$X_1 = \frac{ee'}{r^2}\left[\cos\,(rx)\left\{1 - \frac{(\mathbf{v}\mathbf{v}')}{c^2}\right\} + \frac{v_r v_x'}{c^2}\right].$$

This is the x-component of

$$e[e'\mathbf{r}_1/r^2 + e'/c^2r^2\,.\,V\mathbf{v}V\mathbf{v}'\mathbf{r}_1] = e[\mathbf{E} + c^{-1}V\mathbf{v}\mathbf{H}],$$

where $\mathbf{E} = e'\mathbf{r}_1/r^2 = -\,\nabla_0(e'/r)$

and

$$\begin{aligned}\mathbf{H} &= e'/cr^2\,.\,V\mathbf{v}'\mathbf{r}_1\\ &= -\,e'/c\,.\,V\mathbf{v}'\nabla_0\frac{1}{r}\\ &= \mathrm{curl}_0\,(e'\mathbf{v}'/cr).\end{aligned}$$

Obviously we can add the results for any number of moving charges e'. But there remains the other component :

$$X_2/(ee'/r^2) = \frac{3-\lambda}{4}\frac{v^2}{c^2} - \frac{1-\lambda}{2}\frac{(\mathbf{v}\mathbf{v}')}{c^2} - \frac{3(1-\lambda)}{4}\frac{v_r v_r'}{c^2}$$
$$-\frac{3(1-\lambda)}{4}\frac{v_r^2}{c^2} - \frac{1-\lambda}{2}\frac{v_r v_x'}{c^2} + \frac{1+\lambda}{2}\frac{v_x v_r'}{c^2} - \frac{1+\lambda}{2}\frac{v_x v_r}{c^2}$$

The existence of this latter component means that the formula does *not* hold in Ritz's theory. But it remains true that the force between two charges at relative rest is $e\mathbf{E}$ and also that the force exerted on a moving charge by a neutral closed current is $e/c\,.\,V\mathbf{v}\mathbf{H}$. To prove this latter formula put $\mathbf{v}' = 0$ and $\mathbf{u} = \mathbf{v}$ in (12.13) :

$$\begin{aligned}X_1 &= J'vds'/cr^2\,.\,[\cos\,(vr)\cos\,(xds') - \cos\,(rx)\cos\,(rds')]\\ &= x\text{-component of } J'/cr^3\,.\,[d\mathbf{s}'(\mathbf{r}\mathbf{v}) - \mathbf{r}(\mathbf{v}d\mathbf{s}')],\end{aligned}$$

that is, of

$$J'/cr^3\,.\,V\mathbf{v}Vd\mathbf{s}'\mathbf{r}.$$

Also

$$X_2 = (1+\lambda)J'v_x/cr^2\,.\,ds'(\cos\,rds').$$

The latter term when integrated over the circuit gives zero. From the former

$$\mathbf{F} = c^{-1} V \mathbf{v}\mathbf{H}, \text{ where } \mathbf{H} = J' \int r^{-3} V d\mathbf{s}'\mathbf{r}.$$

Turning now to Liénard's formula (7.17), neglecting v'^2/c^2 and the acceleration terms, we find the formula

$$\mathbf{F} = e(\mathbf{E} + c^{-1} V \mathbf{v}\mathbf{H}),$$

for the force exerted on e moving with $\mathbf{v}$ by e' moving with $\mathbf{v}'$, where $\mathbf{H}$ is the curl of $e'\mathbf{v}'/cr$. Unless therefore we confine the formula to the $\mathbf{H}$ produced by a *neutral* current, it presupposes absolute velocities. That is, it is incompatible with a force-formula which involves only the relative velocity of the two point-charges.

The formula (4.31a) for the force on a unit stationary charge is

$$\begin{aligned} \mathbf{E} &= -\nabla\varphi - c^{-1}\dot{\mathbf{A}} \\ &= -\nabla\frac{e'}{r} - \frac{1}{c}\frac{\partial}{\partial t}\frac{e'\mathbf{v}'}{cr} \\ &= e'\mathbf{r}_1/r^2 - e'/c^2r^2 \,.\, (r\mathbf{f}' - v_r'\mathbf{v}'). \end{aligned}$$

Hence, if we neglect terms containing f' and v'^2, we are justified in taking $\mathbf{E} = e'\mathbf{r}_1 . /r^2$, as we have done above. It is also obvious that formula (4.31a) cannot be proved from experiments on electromagnetics.

5. Moving Circuit and Charge.

Suppose the circuit s' and the charge $e = +1$ are moving with constant velocities of translation, $\mathbf{v}'$ and $\mathbf{v}$ respectively. We can tabulate the following results according to Lorentz and Ritz, where $\mathbf{R}$ or $\mathbf{F}$ denotes the force exerted by the current on the charge, $\mathbf{R}'$ or $\mathbf{F}'$ denotes the force of the charge on the current, χ is $(\mathbf{v}\mathbf{A})/c$ and χ' is $(\mathbf{v}'\mathbf{A})/c$.

Lorentz	*Ritz*
$\mathbf{R} = \nabla\chi - c^{-1}(\mathbf{v}\nabla)\mathbf{A} + c^{-1}(\mathbf{v}'\nabla)\mathbf{A}$	$\mathbf{F} = c^{-1} V(\mathbf{v} - \mathbf{v}')\mathbf{H}$
$\quad = c^{-1} V\mathbf{v}\mathbf{H} + c^{-1}(\mathbf{v}'\nabla)\mathbf{A}$	$\quad = \nabla\chi - \nabla\chi' - c^{-1}(\mathbf{v}\nabla)\mathbf{A} + c^{-1}(\mathbf{v}'\nabla)\mathbf{A}$
$\mathbf{R}' = -\nabla\chi$	$\quad = \mathbf{R} - \nabla\chi'$
	$\mathbf{F}' = -\mathbf{F}$ (12.24)

	Lorentz	*Ritz*	
(1) $\mathbf{v}' = 0$			
	$\mathbf{R}_1 = c^{-1}V\mathbf{v}\mathbf{H}$	$\mathbf{F}_1 = c^{-1}V\mathbf{v}\mathbf{H}$	
	$\mathbf{R}_1' = -\mathbf{R}_1 + c^{-1}(\mathbf{v}\nabla)\mathbf{A}$	$\mathbf{F}_1' = -\mathbf{F}_1$	(12.24a)
(2) $\mathbf{v} = 0$			
	$\mathbf{R}_2 = \nabla\chi' - c^{-1}V\mathbf{v}'\mathbf{H}$	$\mathbf{F}_2 = -c^{-1}V\mathbf{v}'\mathbf{H}$	
	$\mathbf{R}_2' = 0$	$\mathbf{F}_2' = -\mathbf{F}_2$	(12.24b)
(3) $\mathbf{v}' = \mathbf{v}$			
	$\mathbf{R}_3 = \nabla\chi$	$\mathbf{F}_3 = 0$	
	$\mathbf{R}_3' = -\nabla\chi$	$\mathbf{F}_3' = 0$	(12.24c)

The following principle [4] is asserted by followers of Lorentz: 'Any system of electric currents when in motion will, in virtue of a redistribution of its charges, exert a force on a resting charge in its vicinity given by $\mathbf{E} = c^{-1}V\mathbf{v}\mathbf{H}$.' Now for the simple case of uniform linear velocity Lorentz's theory gives, dropping the dashes in (12.24b), $\mathbf{R}_2 = \nabla\chi - c^{-1}V\mathbf{v}\mathbf{H}$, while the reaction of the charge is $\mathbf{R}_2' = 0$. For the case of a charge moving with $-\mathbf{v}$ and a resting circuit, the same theory gives $\mathbf{R}_1 = -c^{-1}V\mathbf{v}\mathbf{H}$ while the reaction of the charge is $\mathbf{R}_1' = \nabla\chi$. Hence we cannot admit as logically consistent with Lorentz's theory the 'simplified principle of relativity' which technology is alleged by Becker (p. 336) to employ: 'A moving [magnetised] bar exerts on a stationary charge the same force as a stationary bar exerts on a charge moving with $-\mathbf{v}$.' The principle holds only for Ritz's theory which such authors hardly mention.

Turn now to case (12.24c), circuit and charge moving together with a common velocity of translation. According to Ritz the force between them is zero. According to both Clausius and Lorentz, the force on the charge is $\mathbf{R} = \nabla\chi$, or

$$R_x = -\int \sigma ds' \cos(rx) \,.\, /r^2, \tag{12.24d}$$

where $\sigma = v/c \,.\, J' \cos(vds') = (\mathbf{J}'\mathbf{v})/c$. That is, the force on e is the same as if caused by a linear distribution of density $-\sigma$ along the circuit. But this is true only to the first order in v/c. For

[4] Tate, p. 79. Why should a redistribution of electricity take place in the circuit, when it is not (according to Lorentz) acted on by the electrostatic charge?

if e and $e' = \sigma ds'$ are moving with a common velocity v, the force on e is, according to Lorentz (7.17),

$$dR_x/ee' = \cos(rx) \,.\, /r^2 \,.\, (1 - v^2/c^2 - 3v_r^2/c^2) + v_x v_r/c^2r^2.$$

Now the force exerted by the charge on the circuit is $-\nabla\chi$; and it is argued that this produces an electrostatic distribution $+\sigma$ on the circuit. The argument was first advanced by Budde in 1880 in answer to an objection of Fröhlich, and more explicitly as follows in 1887 [5]:

> Each ds' behaves towards e as if it had a free charge $-\sigma ds'$. Thus in the circuit s' there is an electrostatic potential just as if each ds' had the charge $-\sigma ds'$. According to a general principle which may be called the principle of the neutralising charge, this potential must be brought into equilibrium by each element ds' assuming an electrostatic charge whose effect is equal and opposite to the imagined charge $-\sigma ds'$.

This principle is by no means as self-evident as its rather pompous enunciation implies. For the force on e, which is really exerted by the current-ions, is only algebraically equivalent to what would be exerted by a different system, i.e. by a distribution $(-\sigma)$ moving rigidly with the circuit. Moreover, this equivalence holds only to the first order in v/c. The charge e exerts an equal and opposite force on the current-ions. But, on Lorentz's theory, this equality of the reaction holds only when the circuit and the charge are moving together. We are now told that this reaction on the ions produces a distribution $+\sigma$ moving rigidly with the circuit, which cancels the force of the current on the charge e, and thus makes the reaction zero. To put it mildly, this alleged principle is not very clear. It was invented to bolster up the hypothesis of a stationary aether, so that there should be no force between a charge and a circuit both at rest in a laboratory. If we assume an earth-convected aether, it therefore becomes superfluous; it is obviously quite unnecessary on Ritz's theory.

Moreover, if we accept Budde's argument for comoving circuit and charge, it ought to be applicable at any moment to the case

[5] Fröhlich, i. 261; Budde, i. 558. The quotation is from Budde, iii. 112 f.*. *Cf.* FitzGerald in 1882 (p. 115). Budde's argument is accepted as valid by the following: Whittaker, p. 263; Lorentz, ii. 41; Silberstein, p. 272; Barnett, vii. 1114; Liénard, iii. 3. Note that the total charge of compensation $\int \sigma ds'$ is zero. Note also that $\mathbf{dR}$ (and $\mathbf{r}$) is drawn from ds' to e, hence a positive R_x acting on e indicates repulsion.

of a stationary circuit and a moving charge, for which $\mathbf{R}_1 = \nabla\chi + c^{-1}(\mathbf{v}\nabla)A$ and $\mathbf{R}_1' = -\nabla\chi$. The argument would result in eliminating the terms $\mp \nabla\chi$. The force on the charge would then be $c^{-1}(\mathbf{v}\nabla)\mathbf{A}$, and the force on the circuit would be zero. Which is certainly incorrect.

Accordingly we regard the following argument of Lorentz as a mere gratuitous *ad hoc* invention :

> Imagine an electric current flowing in a closed circuit without resistance. Would this current act upon a particle carrying a charge which is placed in its neighbourhood ? . . . The answer to this question was of course that the current did not act upon the particle. It would act upon a magnetic needle placed in the neighbourhood, since it is surrounded by a magnetic field ; but there is no trace of an electric field. This is certainly correct so long as the current and the electric particle are at rest [in the aether].
>
> Suppose, however, that both share in some motion, e.g. the earth's motion [i.e. assuming that the laboratory *has* a motion through the aether]. What then ? To begin with, the charged particle will move with a certain velocity through the magnetic field of the current and it will thus be acted upon by some force. It was already stated by Budde that as a consequence of its motion the current will act upon itself, that is to say, upon the electricity in the circuit. Similarly, as through electrostatic influence in a metal, there should be in the circuit a separation of positive and negative electricity; in other words, charges should be produced. Budde added, however, that these charges would be so distributed that their action upon the electrified particle would be just compensated by that of the magnetic field.—Lorentz, xiii. 306.
>
> A conducting wire, traversed by an electric current, assumes a certain charge, positive in one part and negative in another, by the very fact of its translation through the aether. The absence of first-order effects is due to this charge, which I have called compensation-charge.—Lorentz, xvii. 456.

This type of reasoning—' a magnetic field ' and so on—has been completely ousted by the electron theory. We shall presently see that the assertion which Lorentz makes ' of course ' concerning a current and an electrostatic charge is incorrect. His proof of the second assertion, concerning a comoving circuit and charge, is as follows. The motion being steady, we have

$$0 = d/dt = \partial/\partial t + (\mathbf{v}\nabla).$$

Hence

$$c \operatorname{curl} \mathbf{E} = -\dot{\mathbf{H}} = (\mathbf{v}\nabla)\mathbf{H}.$$

Since div $\mathbf{H} = 0$, the solution is clearly, by (1.6),

$$\mathbf{E} = -c^{-1} V\mathbf{v}\mathbf{H}.$$

Hence

$$\mathbf{F} = \mathbf{E} + c^{-1} V \mathbf{v} \mathbf{H} = 0.$$

'Thus there is no resultant force upon the particle.' Also the volume-density in the circuit is given by

$$\rho = \frac{1}{4\pi} \operatorname{div} \mathbf{E} = (\mathbf{u}\mathbf{v})/c^2,$$

if we utilise (1.5) and remember curl $\mathbf{H} = 4\pi \mathbf{u}/c$. 'This space-charge is identical with that already found by Budde.'

This alleged proof suggests the following comments :

(1) Lorentz starts by assuming not only that there is no force on $e = +1$ due to the *total* circuit but also that each element exerts a zero force on e. He has no right to make such an assumption, when he accepts (implicitly or equivalently) Liénard's force-formula, according to which even two uniformly moving charges, at relative rest, exert forces on one another.

(2) His problem is therefore hypothetical : *Assuming* that no force is exerted, what *extra* charge-density (ρ') must be excogitated as moving with the circuit-velocity ($\mathbf{v}$), in addition to the neutral current (ρ with $\mathbf{v}$ and $-\rho$ with $\mathbf{v} - \mathbf{w}$) ? We have the current-density

$$\begin{aligned} \mathbf{u} &= \rho\mathbf{v} - \rho(\mathbf{v} - \mathbf{w}) + \rho'\mathbf{v} \\ &= \rho\mathbf{w} + \rho'\mathbf{v}. \end{aligned}$$

The density $\rho - \rho + \rho'$, i.e. ρ', has been proved equal to $(\mathbf{u}\mathbf{v})c^{-2}$. Hence

$$\rho'(1 - v^2/c^2) = \rho(\mathbf{w}\mathbf{v})/c^2.$$

Or, for linear circuits,

$$\sigma' = (\mathbf{j}\mathbf{v})c^{-2}/(1 - v^2/c^2).$$

That is, if a charge is distributed along the circuit at this linear rate, it will counteract the force exerted on e by the neutral current.

(3) At first sight, this result appears to hold to any order.[6] But this is a delusion due to the fact that Lorentz, as he himself points out, 'assumes that there are no discontinuities.' Once we assume the electron theory, we find—in accordance with the treatment given above—that the result only holds to the first order in v/c ; for velocity-terms are introduced by the Liénard formulae for potential and force.

[6] Fröhlich (ii. 123) already gave the factor $(1-v^2/c^2)^{-1}$, and it was accepted by Budde (i. 645).

(4) Therefore all that Lorentz has done—following Fröhlich and Budde—is to give an unsatisfactory proof of the proposition that a charge distributed at the rate $\sigma = (\mathbf{j}'\mathbf{v})c^{-2}$ *would counteract* the force exerted by the neutral current. We have already shown this in formula (12.24d), when we pointed out that this force was equivalent to that produced by a linear distribution $-\sigma$.

(5) But Lorentz has given no reason whatever for believing that such a compensating charge-distribution comes physically into existence. And even if he had shown it, the result would be no help to those of 'relativist' mentality, inasmuch as we should then have an effect of absolute motion.

(6) The misplaced ingenuity of Budde, adopted by Lorentz and even yet accepted by writers who call themselves relativists, is directed towards inventing *ad hoc* a mysterious physical effect whose sole purpose is to enable those who uphold a stationary aether—i.e. v and v' measured with reference to the fixed stars—to escape from an unpleasant consequence of their theory.

Now in fact it is universally taken for granted that no force of this magnitude can, compatibly with experimental results, exist between a charge and a circuit which are at rest in the laboratory. The obvious conclusion is that $v = 0$, i.e. the electromagnetic framework (or aether) is comoving with the laboratory. And, in a typically contemporary roundabout way, this is admitted by Lorentz and by other relativists :

> All this holds for an observer who sees the circuit in motion and with v^2/c^2 neglected. An observer moving with the circuit would find everything exactly as in a fixed circuit. This of course is required by the principle of relativity ; for such an observer there is no charge [i.e. no space-density ρ].—Lorentz, xiii. 308.

As we are dealing only with the experimental science known as physics, the only relevant 'observer' is the man-in-the-lab. Lorentz admits that for him a circuit at rest in the laboratory has zero velocity ; this used to be called the theory of an earth-convected aether. We are not in the least interested in the mythical being who is hurtling through the laboratory and therefore 'sees the circuit in motion.' We are, however, interested in the case of a circuit and a charge comoving with respect to the electromagnetic framework (the laboratory). Lorentz thinks there is no force but that a space-charge emerges ; but a logical development of the electron theory shows the invalidity of his proof.

6. Einstein.

These few remarks on the alleged relevance of Einstein's views suggest the advisability of a few further comments. Let us examine the case of a magnet (which we represent by the circuit s') and a conductor (which will be represented by a charge $e = +1$). From formulae (12.11) and (12.18) we deduce the following particular cases :

(1) s' at rest ($\psi = \partial\mathbf{A}/\partial t = 0$) and e moving :

$$\mathbf{F}_1 = c^{-1} V\mathbf{v}\mathbf{H}.$$

(2) s' moving and e at rest ($\mathbf{v} = 0$) :

$$\mathbf{F}_2 = \nabla\psi - c^{-1}\partial\mathbf{A}/\partial t.$$

(3) s' and e moving rigidly ($\mathbf{dA}/dt = V\boldsymbol{\omega}\mathbf{A}$) :

$$\mathbf{F}_3 = \nabla(\chi + \psi).$$

Suppose that $\mathbf{v}'$ of the circuit in case (2) is minus the $\mathbf{v}$ of the charge in case (1), then it is not in general true (owing to accelerations) that, on either theory, $\mathbf{F}_2 = -\mathbf{F}_1$. But if the motion in both cases is one of uniform translation and $\mathbf{v}' = -\mathbf{v}$, then, as we have already seen, the relation $\mathbf{F}_2 = -\mathbf{F}_1$ holds for Ritz but not for Lorentz (12.24a and b).

With this background prepared, we are now in a position to quote some assertions of relativist writers, beginning with the opening passage of Einstein's famous paper of 1905 :

> It is known that Maxwell's electrodynamics—as usually understood at the present time—when applied to moving bodies, leads to assymmetries which do not appear to be inherent in the phenomena. Take for example the reciprocal electrodynamic action of a magnet and a conductor. The observable phenomenon here depends only on the relative motion of the conductor and the magnet; whereas the customary view draws a sharp distinction between the two cases in which either the one or the other of these bodies is in motion. For if the magnet is in motion and the conductor at rest, there arises in the neighbourhood of the magnet an electric field with a certain definite energy, producing a current at the places where parts of the conductor are situated. But if the magnet is stationary and the conductor in motion, no electric field arises in the neighbourhood of the magnet. In the conductor, however, we find an electromotive force, to which in itself there is no corresponding energy; but which gives rise—assuming equality of relative motion in the two cases discussed—to electric currents of the same path and intensity as those produced by the electric forces in the former case.—Einstein, p. 37.

It can be deduced from these equations [of Maxwell] that if a magnet is moved in the neighbourhood of a conducting circuit, an electric field will be created around the magnet and will set up a current in the conductor; but if the magnet remains still and the conductor is moved, though indeed the current will appear as before, no electric field will be produced around the magnet. It is, however, difficult to believe that Nature would actually behave in this one-sided way, distinguishing arbitrarily between the motion of the magnet and the motion of the conductor. . . . When Einstein applied this principle [the Voigt transformation], he found that the electric and magnetic forces grouped themselves in the equations in such a way that the discrepancy we have referred to disappeared. This happy result must be regarded as strongly confirmatory of the soundness of his whole argument.—Sir T. P. Nunn, *Relativity and Gravitation*, 1923, p. 28.

All the theorems here deduced, which refer to the motion of a body in a constant magnetic field, are equally valid according to the Principle of Relativity . . . for a body at rest towards which a magnet moves.—Planck, p. 192.

According to Einstein's theory, it is obvious that there is no dissymmetry between the two cases, for in both there is produced the same electric field in the system of reference bound to the conductor. —J. Becquerel, *Le principe de relativité*, 1922, p. 86.

In order to criticise these statements, it is necessary to know only one point in Einstein's theory: namely, that for experiments in a scientific laboratory he accepts Lorentz's aether-electron theory. Everything else is irrelevant, but *this* is vital. The language in which this admission is couched has nothing whatever to do with the scientific formulae. We may, if we like, speak of velocities relative to the 'observer,' instead of velocities relative to the earth-convected aether. We may use all kinds of subjectivist expressions; we may use dashed or primed letters of the alphabet to denote laboratory-measures, as if somehow they were ontologically inferior to unprimed ratios measured by an imaginary observer. We are not now concerned with these ideas; we are interested only in the fact that Einstein accepts Lorentz's theory 'as measured by an observer' in the laboratory. Accepting this theory, he has no escape from the failure of the principle of action-reaction here as in the fundamental formula (7.17). If, as Einstein says, 'the observable phenomenon here depends only on the relative motion of the conductor and the magnet,' then he is logically bound to accept Ritz's theory. There is no use in trying to escape this ineluctable conclusion by flying, with Becquerel, to what would be observed by a hypo-

thetical observer 'bound to the conductor' in its motion. The scientific measurer knows nothing of this imaginary being.

Nor need we waste time in discussing what is alleged to be 'the customary view.' All these distinctions between electric and magnetic fields, the production of e.m.f., and so on, belong to an epoch which is, or ought to be, dead and gone. From the point of view of the Gauss-Fechner-Weber synthesis, there is nothing but forces between moving electrons. We all profess to believe that this is how Nature actually behaves; but in practice we make a fetish of distinctions within this objective complex, which are introduced solely for convenience in dealing with particular cases.

Relativists appear to be under a serious delusion concerning the history of electrical theory. 'According to the older view,' says Dr. N. R. Campbell (iv. 45), 'electrostatics and electrodynamics were two separate and independent studies. . . . But our view is that they are merely aspects of the same thing.' It would not be at all easy to say what is the 'thing' of which they are 'aspects.' But it was made clear a century ago that electrostatics is a particular case of electrodynamics; if in the meantime we have forgotten this, the really old view, the fault must be attributed to the influence of Maxwell.

That Einstein's followers cannot easily rid themselves of this influence is shown by the following quotation from Sir Arthur Eddington (p. 22):

> Consider an electrically charged body at rest on the earth. Since it is at rest it gives an electric field but no magnetic field. But for the nebular physicist it is a charged body moving at 1000 miles a second. A moving charge constitutes an electric current which in accordance with the laws of electromagnetism gives rise to a magnetic field. How can the same body both give and not give a magnetic field? On the classical theory we should have to explain one of these results as an illusion. . . . On the relativity theory both results are accepted. Magnetic fields are relative.

Cutting out the vivid popularising references to the nebular physicist who now replaces the man in the moon of our childhood, let us confine our attention to the pseudo-conundrum, How can the same charge give and not give a magnetic field? The answer is: a magnetic field is never 'given' by a single charge. A magnetic field is merely a mathematical manipulation of the force exerted by a moving charge on a collection of moving

charges which we call a current. As to the charge at rest on the earth, we have the following choice : (1) it is moving through the aether, (2) it is at rest in the aether, (3) it is moving relatively to the current without any reference to an aether. Clearly Eddington rejects (1), so his choice is confined to (2) or (3).

Similarly d'Abro (p. 144), speaking of Rowland's experiments, says that 'classical science assumed that motion in this case meant motion with respect to the stagnant ether.' Thus by 'classical science' he means the view, never fully accepted even by Maxwell, which was advocated by Lorentz and others during the present generation. He adds :

> Once again, the only type of velocity which appeared to have any significance in nature was relative velocity, and never velocity through the stagnant ether or absolute space. Hence Einstein postulated his special principle of relativity, according to which Galilean motion through the ether or space is meaningless.

We have already shown that Rowland's experiments are equally explicable on the theory of an earth-convected aether or on the ballistic theory. The language of this, as of other relativist writers, seems at first sight to class them as followers of Ritz. Not at all ; they accept all the aether formulae. The reference to relative velocity, *alias* velocity relative to the laboratory or 'observer,' is a quibble. It is identical with what *we* call velocity relative to the earth-convected aether ; it enters into the formulae, e.g. that for the force between two moving electrons (7.17), as an *absolute* velocity. All our statements about, and our conclusions from, the Liénard-Schwarzschild force-formula must be accepted by Einstein and his adherents.

7. Induction.

Suppose we have two closed circuits in motion. According to the formulae of Clausius, Lorentz and Ritz, the force exerted by s' on a charge $+1$ moving with the circuit s is of the form

$$F_x = -\frac{\partial L}{\partial x} + \frac{d}{dt}\frac{\partial L}{\partial v_x}.$$

And according to each of the three theories

$$\frac{\partial L}{\partial v_x} = -\frac{1}{c}\int J'\frac{dx'}{r}.$$

Hence the induced e.m.f. is

$$
\begin{aligned}
V &= \int \Sigma F_x dx \\
&= \frac{d}{dt}\int \Sigma \frac{\partial L}{\partial v_x} dx \\
&= -\frac{1}{c}\frac{d}{dt}\iint J' \frac{\Sigma dx dx'}{r} = +\frac{d\Pi}{dt},
\end{aligned} \tag{12.25}
$$

where $\Pi \equiv -\iint J'\,(\mathbf{ds}\,\mathbf{ds}')/r.$

Or, putting it otherwise, according to (12, 11, 18),

$$\mathbf{F} = \nabla(\chi + \psi) + c^{-1} V\boldsymbol{\omega}\mathbf{A} - c^{-1}\mathbf{dA}/dt.$$

Hence

$$
\begin{aligned}
V &= \oint (F\mathbf{ds}) \\
&= -c^{-1}\oint \mathbf{ds}(\mathbf{dA}/dt - V\boldsymbol{\omega}\mathbf{A}) \\
&= -\frac{1}{c}\frac{d}{dt}\oint(\mathbf{A}\mathbf{ds}) \text{ by } (1.34) \\
&= -\frac{1}{c}\frac{d}{dt}\int(\mathbf{H}\mathbf{dS}) \text{ by Stokes's theorem} \\
&= -\frac{1}{c}\frac{dN}{dt} \text{ or } -\frac{1}{c}\left(\frac{\partial N}{\partial t} + \frac{\delta N}{\delta t}\right),
\end{aligned} \tag{12.26}
$$

where N is the magnetic flux through a surface S bounded by the circuit s, the flux and the circulation (or current) being related in a right-handed or positive manner. This is the ordinary formula (4.30) for induction, which is thus given correctly by the three theories for two closed circuits moving in any manner. The notation $\partial N/\partial t$ denotes the rate of change of N if the circuit were at rest, whereas $\delta N/\delta t$ denotes the rate of change due to the motion.

Now by (12.11 or (12.18)

$$\mathbf{F} = \nabla\psi + c^{-1} V\mathbf{v}\mathbf{H} - c^{-1}\partial\mathbf{A}/\partial t$$

and

$$
\begin{aligned}
\delta t \int(\mathbf{ds}\, V\mathbf{v}\mathbf{H}) &= \delta t \int(\mathbf{H}\, V\mathbf{ds}\mathbf{v}) \\
&= \int(\mathbf{H}\mathbf{dS}') \\
&= \delta N',
\end{aligned}
$$

where $\delta\mathbf{s} = \mathbf{v}\delta t$, $V\mathbf{ds}\delta\mathbf{s} = \mathbf{dS}'$ is the directed element of area swept out by ds in time δt, and $\delta N'$ is the flux swept out by the circuit s in time δt. Hence

$$\oint(\mathbf{Fds}) = c^{-1}\delta N'/\delta t - c^{-1}\partial N/\partial t.$$

Therefore

$$\delta N'/\delta t = -\delta N/\delta t. \tag{12.27}$$

We must therefore carefully distinguish between N the flux through a surface bounded by the circuit, and N' the flux swept through by the circuit. When the inducing circuit s' is stationary, so that $\partial N/\partial t = 0$, we have

$$V = -c^{-1}\delta N/\delta t = c^{-1}\delta N'/\delta t,$$

and, if j is the current in elst, the rate of work is

$$jV = -J\delta N/\delta t.$$

On the other hand, the force $\mathbf{F}$ acting on $+q$ moving with $\mathbf{v}$ and on $-q$ moving with $\mathbf{v}-\mathbf{w}$, gives a force

$$q/c\,.\,V\mathbf{wH} = JV\mathbf{dsH}.$$

Hence the work of the ponderomotive forces in time δt is

$$\begin{aligned} J(\delta\mathbf{s}\,V\mathbf{dsH}) &= -J(\mathbf{H}V\mathbf{ds}\;\delta\mathbf{s}) \\ &= -J(\mathbf{HdS}'). \end{aligned}$$

Or, the rate of working for the whole circuit is

$$-J\delta N'/\delta t = J\delta N/\delta t,$$

which is minus the rate of working of the electromotive forces.

Let us now consider the case in which self-induction is involved, i.e. $\mathbf{A}$ is due to the circuit itself. As we did in proving formula (1.34), we shall divide the rate of change of $\oint(\mathbf{Ads})$ into two parts. (1) The first (fig. 54) is the change of $\mathbf{A}$ at each point of the circuit s due to the fact that in the interval δt the circuit has moved to s'. This portion of the rate of change is given by

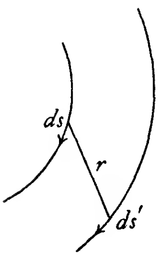

Fig. 54.

$$\frac{1}{\delta t}\int(\mathbf{A}'\mathbf{ds}) = \frac{j/c}{\delta t}\iint\frac{(\mathbf{ds}\;\mathbf{ds}')}{r},$$

which we may express as

$$\frac{\partial N}{\partial t} = J\frac{\partial L}{\partial t}.$$

(2) The second part is due to the fact that in the interval δt the path of integration changes from s to s'. The rate of change is given by

$$\frac{1}{\delta t}\int(\mathbf{A}d\mathbf{s}') = \frac{j/c}{\delta t}\iint\frac{(d\mathbf{s}\,d\mathbf{s}')}{r},$$

which we may express as

$$\frac{\delta N}{\delta t} = J\frac{\partial L}{\partial t}.$$

Hence

$$\frac{dN}{dt} = 2\frac{\partial N}{\partial t} = -\,2\frac{\delta N'}{\delta t}.$$

Thus what Dunton (p. 446) calls 'the true law of electromagnetic induction' is, if we measure V in elms, given by

$$V = -\,dN/dt,$$

which apparently is what engineers call the rate at which the flux-linkage is being decreased. It is certainly *not* given by the so-called 'cutting rule'

$$V = -\,\delta N'/\delta t.$$

Apart from the sign which is wrong, this gives (in case of self-induction in moving circuits) only *half* the correct result.[7]

Let us illustrate this by considering the circuit already discussed in Chapter IV. Figure 55 represents a long rectangular

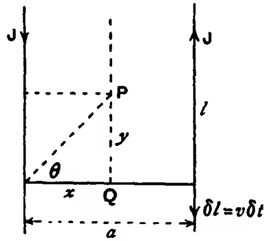

Fig. 55.

[7] *Cf.* Hirst's text-book published in 1936 (p. 196): 'The e.m.f. is equal to the rate at which the conductor cuts the flux, or to the rate of change of the flux linked with the turn.' The usual description of N as 'the total number of tubes of induction which cut the circuit' (Jeans, p. 453) is ambiguous. 'The elm unit of potential . . . is the potential difference produced by a given rate of cutting of lines of force. . . . It can be shown that it is not the rate of change of flux, but the cutting of the lines of force associated with the rate of change of flux, which causes the e.m.f.—Loeb (1931), p. 242. 'During the last thirty years, the question as to whether electromagnetic induction is caused by the change or by the cutting of magnetic flux has been debated at intervals. Faraday's rotating disk experiment and later experiments . . . all appear definitely to favour the flux-cutting hypothesis.'—C. V. Drysdale, *Nature* 141 (1937), 254. 'None of these [experiments] can be explained by the rate of change of [magnetic] induction through the circuit.'—Cramp and Norgrove (1936), p. 489. Cf. the controversy in the *Electrician*, vols. 75 and 76 (1915).

circuit in which the crosspiece can move downwards ; a/l is very small, so also is b/a, where b is the distance between the vertical and horizontal filaments of current. The magnetic intensity at P due to the left-hand wire is

$$H = J/x \,.\, (1 + \sin\theta).$$

The flux through the strip dx is

$$dN = \frac{Jdx}{x}\int_0^l \left[1 + \frac{y}{\sqrt{x^2 + y^2)}}\right]dy$$
$$= \frac{Jdx}{x}[y + \sqrt{x^2 + y^2}]_0^l$$
$$\rightarrow 2Jldx/x.$$

Doubling this for both wires, we have the total flux

$$N = 4Jl\int_a^{a-b} dx/x$$
$$= 4Jl \log(a/b - 1).$$

Suppose the crosspiece moves down with velocity $v = dl/dt$. The induced e.m.f. in elms is

$$V = -\, dN/dt$$
$$= -\, 4Jv \log(a/b - 1).$$

The area swept out by dx of the crosspiece in time δt is

$$dS' = |\, V\mathbf{dx}\delta\mathbf{l}\,| = -\, dx\delta l = -\, dxv\delta t,$$

the negative sign meaning that the area-vector is pointing down through the paper. The magnetic intensity at the point Q is

$$J[1/x + 1/(a - x)],$$

upwards through the paper. Hence

$$\frac{\delta N'}{\delta t} = \frac{1}{\delta t}\int HdS' = -\, 2Jv \log(a/b - 1).$$

We have thus verified the relation

$$dN/dt = -\, 2\delta N'/\delta t.$$

Also the downward force on the crosspiece is

$$F = J\int |\, V\mathbf{dx}\mathbf{H}\,| = J\int Hdx$$
$$= 2J^2 \log(a/b - 1),$$

in agreement with formula (4.12e) with $a/l \rightarrow 0$.

Most teachers of physics will cordially endorse the statement of Prof. W. L. Bragg (p. 142): 'It has been my experience that students find the idea of electromagnetic induction harder to grasp than any other in electricity and magnetism.' Various attempts have been made to connect this phenomenon with other experimental results. We have already criticised the attempt, initiated by Helmholtz, to prove the law of induction from the law of electrodynamics; and in Chapter IV we reversed this argument and deduced Ampère's law of force (for complete circuits) from the law of induction accepted as an experimental fact. But it is only now that we have been able to marshal the phenomena under a simple synthesis. From the law of inter-electronic force, formulated in two entirely different ways, we have deduced both the force between current-elements and the formula for induction.

By way of contrast let us now take a look at one or two rival attempts. Pidduck (p. 270) gives what he calls the 'electronic theory of induced currents.' If $\mathbf{v}$ is the velocity of the wire at any point and $\mathbf{w}$ the mean velocity of the electrons relative to the wire, the 'apparent electric force' is

$$e\mathbf{F} = e/c \,.\, (V\mathbf{vH} + V\mathbf{wH}).$$

Since $\mathbf{w}$ is along $\mathbf{ds}$, $(\mathbf{ds}V\mathbf{wH}) = 0$. Hence the e.m.f. is

$$V = \int (\mathbf{Fds}) = c^{-1}\int (\mathbf{ds}V\mathbf{vH})$$

$$= c^{-1}\int \mathbf{H}V\mathbf{dsv}$$

$$= c^{-1}\delta N'/\delta t.$$

Which is the erroneous 'cutting rule' already rejected. Pidduck conceals the error by writing $-dN/dt$ instead of $+\delta N'/\delta t$. The reason for his mistake is now apparent. He omitted the last term in the formula

$$\mathbf{F} = \nabla\psi + c^{-1}V\mathbf{vH} - c^{-1}\partial\mathbf{A}/\partial t.$$

Thinking he has correctly deduced the law of induction in a moving circuit, Pidduck remarks:

> The theory will not explain the currents induced in a stationary circuit by a variable magnetic field. This phenomenon shows that a variable magnetic field is accompanied by an electric field, and receives its natural interpretation in terms of the ether.

Further on (p. 409) he develops these metaphors :

> The new theory [i.e. the fictitious displacement-current] throws some light on the problem left unsolved, namely, how induction takes place in a fixed circuit while another circuit is moved. In Maxwell's view, a changing magnetic force is accompanied by an electric force, by the inherent constitution of the intervening medium. The contrast of the mechanisms is one of the least pleasing features of the ether theory and a consequence of its non-relativistic origin.

In reality this 'least pleasing feature' is a subjective delusion generated by failure to develop logically the electron theory. That blessed word 'relativity' has nothing to do with the problem.

Consider the treatment in another recent textbook (Frenkel, i. 124 ff.), in which the author has no hesitation in using very advanced mathematics but lamentably fails to clarify the elementary basis of the electron theory. It is first assumed that the rate of working of the ponderomotive or transverse forces is $J\delta N/\delta t$. To obtain the corresponding expression for the longitudinal or electromotive forces, it is assumed that a force $\mathbf{F} = c^{-1}V\mathbf{vH}$ acts on the negative charge $-q$ which is displaced $-\mathbf{w}\delta t$ in time δt, so that for the whole circuit the rate of working is $cJ\oint(\mathbf{Fds})$. It is next claimed that the sum of these two rates is zero. Thus is proved the law of induction

$$\oint(\mathbf{Fds}) = -c^{-1}\delta N/\delta t,$$

though only for the case when the inducing circuit is at rest. The procedure is rather involved. For if we assume

$$\begin{aligned}\mathbf{F} &= c^{-1}V\mathbf{vH}\\ &= \nabla\chi - c^{-1}(\mathbf{v}\nabla)\mathbf{A}\\ &= \nabla\chi - c^{-1}\delta\mathbf{A}/\delta t,\end{aligned}$$

we can derive the induction formula at once.

Frenkel's next step is to proceed to moving circuits :

> When both circuits s and s' have a common velocity of translation, . . . experience teaches that everything occurs as in a state of rest. It follows that the e.m.f. depends only on the relative motion of s and s'. We shall call this the principle of relativity.

Thus an empirical result due to experiment becomes suddenly exalted into a 'principle.' The really vital presupposition—

that we are dealing with two closed neutral circuits—is forgotten. The author, who accepts Liénard's force-formula, fails to explain to us why *this* force does not depend only on the relative motion of e and e'. His 'principle' is evidently subject to severe limitations.

According to Lorentz-Liénard, when s' is at rest and s is moving with $\mathbf{v}$,

$$\begin{aligned}\mathbf{F} = \mathbf{R}_1 &= c^{-1} V\mathbf{v}\mathbf{H} \\ &= \nabla\chi - c^{-1}(\mathbf{v}\nabla)\mathbf{A} \\ &= \nabla\chi - c^{-1}d\mathbf{A}/dt,\end{aligned}$$

so that

$$V = - c^{-1}dN/dt.$$

And when s' is moving with $-\mathbf{v}$ and s is at rest,

$$\begin{aligned}\mathbf{F} = \mathbf{R}_2 &= - \nabla\chi + c^{-1} V\mathbf{v}\mathbf{H} \\ &= c^{-1}(\mathbf{v}\nabla)\mathbf{A} \\ &= - c^{-1}\partial\mathbf{A}/\partial t,\end{aligned}$$

so that

$$V = - c^{-1}dN/dt.$$

We also have curl $\mathbf{R}_2 = - c^{-1}\partial\mathbf{H}/\partial t$. Writing $\mathbf{E}$ for $\mathbf{R}_2$, Frenkel (i. 130) proceeds to identify this with 'Maxwell's fundamental equation for electromagnetic fields variable in time.' And two pages later he puts

$$\mathbf{E} \text{ (i.e. } \mathbf{R}_2) = - \nabla\varphi - c^{-1}\partial\mathbf{A}/\partial t,$$

where φ 'completely agrees with the scalar or electric potential already introduced.' Then a few lines further on we are told that φ and $\mathbf{A}$ are 'two unknown functions,' so that we can put div $\mathbf{A} = 0$ or div $\mathbf{A} = - \dot{\varphi}/c$ just as we please.

It is surely high time that some logic and clarity were introduced into expositions of electrical theory. As regard the present issue our text-books have not got beyond the view expressed by Bertrand (p. 215):

> We must ask what the law of induction becomes when the two circuits are both in motion. The reply appears obvious *a priori* and experience confirms it.

One more remark remains to be made. In the case of circuits *at rest* (with changing current) the e.m.f. of induction arises solely from the acceleration-terms. In the case of Ritz's formula we have already seen (11.4b) that the acceleration-terms which do

not arise from the expansion in series give zero when integrated round a closed circuit. It follows that the phenomenon of induction in closed circuits in relative rest arises solely from the finite velocity of propagation.[8]

Having confined our treatment to induction in closed circuits, we subjoin the following observation made by Ritz (p. 400):

When a condenser is discharged through a wire, we obtain a first approximation, sufficient in most cases, by calculating the electromagnetic effects . . . and the self-induction *as if the current were closed*, naturally taking into account the electrostatic actions of the condenser-charges. Hence these calculations will continue to be applicable in the new theory; in conformity with experience, they lead to very rapid phenomena in which the accelerations f are very large relatively to the velocities v; for example if there are n sinusoidal oscillations per second, the maximum value of f is $2\pi n$ times that of v. In these experiments the electrostatic term, the resistance and the induction (proportional to $\partial J/\partial t$, i.e. to f), alone play a part as regards the motion of electricity in the conductors. As to the couples exerted on the magnetic needles or coils, we have seen that it is sufficient, for the identity of the theories, that *one* of the currents should be closed, which is in fact the case. The effects of a motion of the conductors, which is always slow in comparison with these phenomena, would not have any sensible influence; more generally, the terms in v', small relatively to those containing f', are without inductive effect in these phenomena. The oscillations of such circuits, often called quasi-stationary, and their effects on neighbouring circuits, will therefore be the same in both theories. It is only when the phenomena become extremely rapid (Hertzian oscillations) that the development in series leading to the formula [11.7] ceases to be very convergent; the propagation then intervenes explicitly.

We may also quote another remark of his (p. 342):

We now know that the energy remains constant only in case there is no radiation; hence the relations which the equation of energy implies between the actions of induction of *open* circuits and their electrodynamic actions may cease to be satisfied. This is indeed what happens; for the phenomena of induction in bodies at rest, the equations of Maxwell-Lorentz and those of Helmholtz

[8] 'As with magnetic forces, inductive forces can be shown to be a necessary outcome of the fact that electric disturbances do not travel instantaneously. The relationship between inductive forces and electric and magnetic forces, however, can only properly be discussed in terms of the theory of relativity.'—Pilley, p. 246. This holds only for Ritz's theory, and then only for circuits at relative rest. The matter has no connection with so-called relativity.

become identical if $\lambda = 0$, as the latter remarked [i. 573]. In this case the resistance, electrostatic force and accelerations alone play a part. Lorentz's formulae are then identical with ours, which equally correspond to $\lambda = 0$. As regards the consequences relative to stability requiring $\lambda \geqq 0$, they are applicable only to the phenomena of induction; to see this, it is sufficient to suppose the currents sensibly zero. Our formulae satisfy this *always*, and our parameter remains *entirely* undetermined.

8. 'Relativity.'

Formula (12.11)

$$\mathbf{R} = \nabla\psi + c^{-1}V\mathbf{v}\mathbf{H} - c^{-1}\partial\mathbf{A}/\partial t$$

is often confused with (4.31b and c):

$$\mathbf{F} = -\nabla\varphi + c^{-1}V\mathbf{v}\mathbf{H} - c^{-1}\partial\mathbf{A}/\partial t.$$

In the former formula, as we see from (7.15), ψ is compounded of the contributions $\mp q'$ moving with different velocities. Also $\mathbf{A}$ is here defined differently, involving only the velocity $\mathbf{w}'$ relative to the conductor.

Maxwell calls this formula (4.31) the equation of intensity 'referred to the fixed axes,' and he has a section (ii. 241) 'on the modification of the equations of electromotive intensity when the axes to which they are referred are moving in space,' i.e. moving relatively to the electromagnetic framework (aether) which experiment shows to be comoving with the earth (at least in its orbital motion). Since

$$\begin{aligned} V\mathbf{u}\mathbf{H} &= V\mathbf{u}\ \mathrm{curl}\ \mathbf{A} \\ &= \nabla(\mathbf{u}\mathbf{A}) - (\mathbf{u}\nabla)\mathbf{A}, \end{aligned}$$

we have

$$\mathbf{F} = -\nabla\varphi' + c^{-1}V\mathbf{v}'\mathbf{A} - c^{-1}\partial\mathbf{A}/\partial t',$$

where

$$\begin{aligned} \mathbf{v}' &= \mathbf{v} + \mathbf{u}, \\ \varphi' &= \varphi - (\mathbf{u}\mathbf{A})/c \\ \partial\mathbf{A}/\partial t' &= \partial\mathbf{A}/\partial t + (\mathbf{u}\nabla)\mathbf{A}. \end{aligned}$$

This is Maxwell's result, simplified by being deduced and expressed vectorially. And this is his conclusion:

> It appears from this that the electromotive intensity is expressed by a formula of the same type, whether the motions of the conductors be referred to fixed axes or to axes moving in space, the only difference between the formulae being that in the case of moving axes the electric potential φ must be changed into φ'.

In all cases in which a current is produced in a conducting circuit, the electromotive force is the line-integral $\int(\mathbf{F}d\mathbf{s})$ taken round the curve. The value of φ disappears from this integral, so that the introduction of φ' has no influence on its value. In all phenomena therefore relating to closed circuits and the currents in them, it is indifferent whether the axes to which we refer the system be at rest or in motion.—Maxwell, ii. 243* (§ 601).

Speaking of this 'important theoretical contribution' of Maxwell, Sir Joseph Larmor says (i. 18):

It is there verified by direct transformation that the type of the equations of electromotive disturbance is the same whether they are referred to axes of co-ordinates at rest in the aether or to axes which are in motion after the manner of a solid body.

Barnett (vii. 1112) regrets that 'Maxwell's theorem' has been 'hitherto but little used.' He thinks however (p. 1113) that it is 'only an approximation.' It 'was derived for the general case involving rotation,' he says (p. 1124), but its application to such a case 'involves the assumption that the tubes of induction rotate with the system—which is inconsistent with Maxwell's general theory.'

Now it seems clear that Maxwell's conclusion and his followers' commentaries are entirely irrelevant and misplaced. For both potentials involve absolute velocities:

$$\varphi = \int de/r \,.\, [1 + w^2/2c^2 + w_r^2/2c^2 - rf_r/2c^2 \; . \; . \; .]$$

$$\mathbf{A} = \int de[\mathbf{w}/cr + \; . \; . \; .].$$

Hence Maxwell's deduction is incorrect. As a matter of fact, however, he is right as regards *neutral* closed circuits. We have already proved this from the Lorentz electron theory; but it does not follow from Maxwell's simple manipulation of formula (4.31).

It is a simple consequence, not only of Ritz's radically relativist theory but also of the absolute theories of Clausius and Lorentz, that forces and induction-effects are, in the case of closed neutral circuits, independent of the absolute velocities of the circuits. Hence it is utterly misleading to cite this as an argument for Einstein's theory.

The e.m.f. depends only on the relative motion of s and s'. We shall call this the principle of relativity.—Frenkel, i. 126.

Induction-effects depend only on the relative motions of magnet and coil. . . . The theorem of the relativity of induction-effects is one of the bases of the modern principle of relativity.—Mie, p. 276.

Quite apart from the negative experiments, Einstein lays special stress on another type of phenomenon. Thus, whether we displace a magnet before a closed circuit or the closed circuit before the magnet, the current induced in the wire is exactly the same in either case so far as experiment can detect. That which appears relevant is the relative motion between magnet and circuit; the respective absolute velocities of magnet and circuit through the ether, which are of course different in both cases, seem to be totally irrelevant.—d'Abro, p. 143 f.

Inasmuch as we have already proved that the e.m.f. induced in a *closed* circuit by another circuit or by a magnet depends only on their *relative* motion, whether we adopt Clausius, Lorentz or Ritz as guide, we naturally reject as fallacious the attempt of relativists to utilise this conclusion as an argument for *their* views.

The following attempt of Mason and Weaver (p. 254 f.) to prove Maxwell's equation c curl $\mathbf{E} = -\dot{\mathbf{H}}$ from an alleged 'relativity principle' must also be pronounced to be a delusion:

Consider two closed circuits 1 and 2; and suppose first that 2 is stationary and is traversed by a current which is maintained constant by some outside influence, while circuit 1 moves with a velocity v. The magnetic field due to the current in circuit 2 is then constant at any point. . . . Suppose now, on the other hand, that circuit 1 is fixed, and circuit 2—in which the current is maintained at its previous constant value—moves with a velocity $-v$. *The actual physical situation, according to a simple relativity principle, is the same in the two cases; in either instance one circuit moves with respect to the other with a velocity of magnitude v; and it is a mere peculiarity of the method of description which one is said to be still and which moving.* In the latter case [1 stationary], however, the electrons of circuit 1, the forces on which are being investigated, are at rest. Thus the motional intensity is zero, as is also the ordinary electrostatic force; since both wires are supposed uncharged. But since the two cases are in reality identical, it must be concluded that there is a force causing the electrons of circuit 1 to move, the total e.m.f. around this circuit being the same as before.

From the words we have italicised we might infer that the authors were adherents of Ritz's relativistic radicalism: an impression which is confirmed by the title 'Fig. 49.—Two circuits in uniform relative motion.' Yet they utilise this premiss for the alleged deduction of a non-relativistic formula; and later on (pp. 296 f.) they actually give Liénard's force-formula which is expressed in terms of absolute or non-relative velocities.

The origin of this confusion lies in the negligence of our text-book writers to deduce the force between two moving circuits and the e.m.f. induced in a moving circuit from whatever force-formula they adopt. Had they done so, they would have seen that these 'relativistic' phenomena are quite compatible with the 'absolutistic' force-formula which—though they seem unaware of it—practically all of them adopt.

9. A Rotating Coil.

Let us write down the expressions previously obtained for the force exerted on a resting charge by a moving circuit, which we take to be a rotating circular coil so that we put $ds' = ad\theta$ and $v' = a\omega$.

According to Lorentz (12.1)

$$R = X_2 = J'ea^2\omega/c \,.\int d\theta/r^2 \,.\, \cos(rx)[\cos(v'ds') - 3\cos(rv')\cos(rds')]. \qquad (12.28)$$

According to Ritz (12.15)

$$F = X_2 = J'ea^2\omega/2c \,.\int d\theta/r^2 \,.\, \cos(rx)\Big[(3-\lambda)\cos(v'ds') - 3(1-\lambda)\cos(rv')\cos(rds')\Big]$$
$$- (1+\lambda)J'ea^2\omega/2c \,.\int d\theta/r^2 \,.\, [\cos(xv')\cos(rds') + \cos(rv')\cos(xds')]. \qquad (12.29)$$

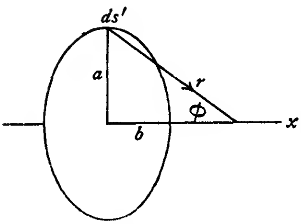

Fig. 56.

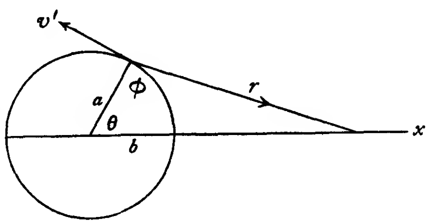

Fig. 57.

First take e at a distance b along the axis (Fig. 56). We have

$$\cos(rx) = \cos\varphi = b/r,$$
$$\cos(rds') = \cos(rv') = 0,$$
$$\cos(v'ds') = 1,$$
$$\cos(xds') = \cos(xv') = 0.$$

Then we easily find that

$$R = 2\pi a^2 b\omega J'e/c(a^2 + b^2)^{3/2}, \tag{12.30a}$$
$$F = (3 - \lambda)R/2. \tag{12.30b}$$

Next (Fig. 57) let us take e in the plane of the coil. We have $(rx) = \pi - (\theta + \varphi)$, $(xds') = (xv') = \pi/2 + \theta$, $(rds') = (rv') = \pi/2 + \varphi$. And from (12.28)

$$R = - J'ea^2\omega/c \,.\int d\theta/r^2 \,.\, \cos(\theta + \varphi)\,(1 - 3\sin^2\varphi).$$

Since $r \sin\varphi = b \sin\theta$ and $a^2 = r^2 + b^2 + 2br\cos(\theta + \varphi)$, the integral is

$$\frac{a^2 - b^2}{2b}\int\frac{d\theta}{r^3} - \frac{1}{2b}\int\frac{d\theta}{r} - \frac{3}{2}b(a^2 - b^2)\int\frac{\sin^2\theta d\theta}{r^5} + \frac{3b}{2}\int\frac{\sin^2\theta d\theta}{r^3}.$$

Now the following results can be proved :

$$\int\frac{d\theta}{r} = \frac{4}{a+b}K,$$
$$\int\frac{d\theta}{r^3} = \frac{4(a+b)}{(a^2 - b^2)^2}E,$$
$$\int\frac{\sin^2\theta d\theta}{r^3} = \frac{2}{a^2b^2}\left[\frac{a^2 + b^2}{a+b}K - (a+b)E\right],$$
$$\int\frac{\sin^2\theta d\theta}{r^5} = \frac{2}{3a^2b^2}\left[\frac{(a^2 + b^2)(a+b)}{(a^2 - b^2)^2}E - \frac{1}{a+b}K\right],$$

where K and E are the complete elliptic integrals of the first and second kind with $k^2 = 4ab/(a+b)^2$.

Whence we obtain

$$R = -\frac{2J'e\omega}{bc}\left[\frac{a^2 + b^2}{a+b}K - (a+b)E\right]. \tag{12.31}$$

This is not zero except in the limiting case when the circular circuit approximates to a long straight wire.

According to Ritz (12.29)

$$F/(- J'ea^2\omega/c)$$
$$= \frac{3-\lambda}{2}\int\frac{d\theta\cos(\theta + \varphi)}{r^2} - \frac{3(1-\lambda)}{2}\int\frac{d\theta}{r^2}\cos(\theta+\varphi)\sin^2\varphi$$
$$+ (1+\lambda)\int\frac{d\theta}{r^2}\sin\theta\sin\varphi.$$

Whence

$$F/(-J'ea^2\omega/4bc)$$

$$= -(3-\lambda)\int\frac{d\theta}{r} + (3-\lambda)(a^2-b^2)\int\frac{d\theta}{r^3}$$

$$+ (7+\lambda)b^2\int\frac{\sin^2\theta d\theta}{r^3} - 3(1-\lambda)b^2(a^2-b^2)\int\frac{\sin^2\theta d\theta}{r^5}.$$

Inserting the values of the integrals, we find

$$F/(-J'e\omega/bc) = [a^2+3b^2+\lambda(a^2+b^2)](a+b)^{-1}K$$
$$-[a^2-3b^2+\lambda(a^2-b^2)](a-b)^{-1}E. \tag{12.32}$$

Thus while it is true, as Whittaker (p. 262) says, that 'if a circular current be rotated with constant angular velocity round its axis, according to Weber's law [and also according to Ritz's law] there would be a development of free electricity on a stationary conductor in the neighbourhood,' the same is true of Lorentz's law. But it is doubtful whether this small effect is accessible to experiment. As a numerical example let us take $b/a = 1\cdot805$ so that $k = \sin 80^\circ$, $K = 3\cdot1534$ and $E = 1\cdot0411$. The Lorentz force is

$$0\cdot6\ J'ev'/bc,$$

where v' is $a\omega$. And, putting $\lambda = 3$, the Ritz force is

$$22\ J'ev'/bc.$$

It is also worth while to calculate ψ. First on Lorentz's theory. Since $v'_x = -\omega a \sin\theta$, $v'_y = \omega a\cos\theta$, $g'_x = dv'_x/ds' = -\omega\cos\theta$, $g'_y = dv'_y/ds' = -\omega\sin\theta$, so that $\mathbf{g}'$ is ω inwards along a and $g'_r = \omega\cos\varphi$. Since $a = r\cos\varphi + b\cos\theta$,

$$\psi = -J'/c\,.\int g'_r ds'$$

$$= -J'a\omega/c\,.\int\cos\varphi d\theta$$

$$= J'a\omega/c\,.\left[b\int r^{-1}\cos\theta d\theta - a\int r^{-1}d\theta\right].$$

Now

$$\int r^{-1}d\theta = 4K/(a+b)$$

$$\int r^{-1}\cos\theta d\theta = 2[(a^2+b^2)K-(a+b)^2E]/ab(a+b).$$

Hence

$$\psi = 2J'\omega/c\,.\,[(b-a)K-(b+a)E].$$

When $a/b = n$ is small, k^2 approximates to $4n(1-2n)$, K to $\pi/2\,.\,(1+k^2/4)$, E to $\pi/2\,.\,(1-k^2/4)$. Hence ψ becomes

$$-4\pi a^2 J'\omega/bc.$$

Similarly in Ritz's theory

$$\begin{aligned}\psi &= -J'/c\,.\int r^{-1}v'ds'\cos(v'ds') + (1-\lambda)J'/2c\,.\int g_r'ds'\\ &= -J'a^2\omega/c\,.\int r^{-1}d\theta + (1-\lambda)J'a\omega/2c\,.\int\cos\varphi d\theta\\ &= -J'\omega/(a+b)c\,.\,[\{2(1+\lambda)a^2+(1-\lambda)(a^2+b^2)\}K\\ &\qquad\qquad -(1-\lambda)(a+b)^2E].\end{aligned}$$

Pegram (p. 597), referring to a spinning solenoid, concludes that 'the whole effect is just that of a current in the stationary solenoid, which is nil on a stationary electron.' Instead of taking the Lorentz equation

$$F = \nabla\psi + c^{-1}V\mathbf{vH} - c^{-1}\partial\mathbf{A}/\partial t,$$

he takes the incorrect but prevalent equation

$$F = -\nabla\varphi + c^{-1}V\mathbf{vH} - c^{-1}\mathbf{dA}/dt.$$

It is not easy to gather what exactly is meant by φ, but it is assumed to be independent of ω. Naturally an argument founded on such premisses is worthless. But the view is nevertheless still quoted as authoritative: 'Pegram points out that on the crudest view of the electron theory of conduction it would be improbable that a solenoid rotating about its axis could exert a force on an electric charge in its vicinity.' [9] It is not clear what is 'the crudest view.' But on Lorentz's theory, which all these writers profess to hold, a spinning coil does exert a force on a stationary charge in its plane or on its axis.

It has also been asserted by Whittaker (p. 263 f.) that 'on the unitary hypothesis that the current consists in a transport of one kind of electricity with a definite velocity relative to the wire, it might be expected that a coil rotated rapidly about its own axis would generate a magnetic field different from that produced by the same coil at rest.' E. L. Nichols and W. S. Franklin, who carried out such an experiment with a negative result in 1889,

[9] Tate, p. 92. *Cf.* Swann (i. 377): 'No electrical effects are to be expected as the result of the rotation of such a solenoid about its own axis.'

declare[10] that 'if the current traversing the coil had possessed direction and a finite velocity, a change in the deflection of the needle might have been looked for as the result of the revolution of the coil.' But the answer to these assertions is quite simple. The magnetic needle is a system of neutral closed currents. The so-called magnetic field is derived from the force between closed circuits. And we have already seen that this force is independent of the velocities of the circuits. Hence no result due to the motion was to be expected; and none was obtained.

10. The Force on an Electrostatic Charge.

We have hitherto neglected the very small force which we have termed X_3 in the theories of Lorentz and Ritz. If we put $v = v' = 0$, $X_1 = X_2 = 0$, and X_3 becomes the force exerted by a stationary current on a stationary charge.

Let us consider a circular current and a charge e on the axis (Fig. 56). We have

$$\cos(rx) = \cos\varphi = b/r,$$
$$\cos(rds') = \cos(xds') = 0.$$

According to Lorentz (12.1) the force (along x) is

$$\begin{aligned} R &= eJ'w'/2c \,.\int ds'/r^2 \,.\cos(rx)[3\cos^2(rds') - 1] \\ &= -\pi abeJ'w'/c(a^2+b^2)^{3/2}. \end{aligned} \tag{12.33}$$

This is a maximum when $b = a/\sqrt{2}$, its value is then

$$R = -eJ'(w'/c)2\pi/3a\sqrt{3}.$$

If $J' = 100$ elm and $w'/c = 10^{-10}$, this force is about $10^{-8}\, e/a$ dyne, where e is in elst and a is in cm.

According to Ritz (12.12),

$$\begin{aligned} F = -J'\frac{w'}{c}\int\frac{ds'}{r^2}\Big[\cos(rx)\Big\{\frac{3-\lambda}{4} - \frac{3(1-\lambda)}{4}\cos^2(rds')\Big\} \\ - \frac{1+\lambda}{2}\cos(rds')\cos(xds')\Big] \\ = \frac{3-\lambda}{2}R, \end{aligned} \tag{12.34}$$

which is zero if $\lambda = 3$.

[10] *Am. J. of Science*, 37 (1889) 103. On p. 109 the authors say they would have been able to observe a deflection due to the motion of the coil even if $w' > 90 \,.\, 10^{3}$ cm./sec.!

In 1877, Clausius (ii. 86) wrote: 'We accept as criterion the experimental result that a closed constant current in a stationary conductor exerts no force on stationary electricity.' It was on account of this assumption that he introduced absolute velocities into his law of force and rejected the formulae of Weber and Riemann. 'It has been shown indeed that the assumption of opposite electricities moving with equal and opposite velocities in a circuit is almost inevitable in any theory of the type of Weber's [*e.g.* that of Ritz], so long as the mutual action of two charges is assumed to depend only on their relative (as opposed to their absolute) motion.'[11] This argument is however invalid, for it assumes that the force of a current on an electrostatic charge must be accurately zero.

But we have just shown that this is not true even on Lorentz's theory. And Budde's type of argument cannot be urged against our formula; for, according to Lorentz, the charge exerts no reaction on the current. The validity of the practically universal assumption that there is no force on a resting charge is at last beginning to be doubted—a rather tardy exhibition of logic on the part of those who profess to hold Lorentz's theory!

> Hitherto it has been almost a principle of faith with physicists that an electric current exerts no force on stationary charges. But it must be admitted that as yet there are no measurements in this direction, and perhaps they cannot be made owing to the extraordinary smallness of the effect.[12]

There may have been grounds for the prejudice so long as w/c was thought to be appreciable; but there is no longer any excuse now that we know that w/c is of the order 10^{-10}.

In the case just investigated both Lorentz's and Ritz's theories gave a positive result[13]; or rather Ritz's formula gave a zero force if (as is probable) $\lambda = 3$. Let us now consider (Fig. 58) the case of a wire B charged with q elsts per unit length,

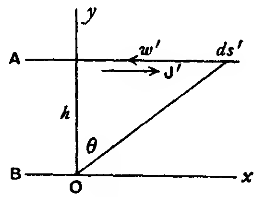

Fig. 58.

[11] Whittaker, p. 231, referring to Lorberg (i). *Cf.* Graetz (p. 828): Absolute velocity is 'a necessary consequence of every law which uses the unitary theory.'

[12] E. Klein, ZfP 77 (1932) 417. Even Clausius (v. 229) called the assumption *Erfahrungssatz.*

[13] That of Clausius always gives a zero result. *Cf.* Clausius (vi. 612): 'The law formulated by me leads to the result that a constant stationary closed circuit exercises no force on a stationary charge.'

while a parallel wire A carries a current $J' = j'/c$ elms. Let us find on Ritz's theory the force exerted by $\mp q'$ on q at O. Since

$$s' = h \tan\theta, \quad ds' = h \sec^2\theta d\theta, \quad ds'/r^2 = d\theta/h,$$

we have

$$-dF_y = -\frac{qq'}{r^2}\cos\theta\left[1 + \frac{3-\lambda}{4}\frac{w'^2}{c^2} - \frac{3(1-\lambda)}{4}\frac{w'^2\sin^2\theta}{c^2}\right] + \frac{qq'}{r^2}\cos\theta$$

$$= -\frac{qq'w'^2}{4c^2r^2}\cos\theta[3 - \lambda - 3(1-\lambda)\sin^2\theta].$$

$$F_y = \frac{qw' . J'}{4c}\int\frac{ds'}{r^2}\cos\theta[3 - \lambda - 3(1-\lambda)\sin^2\theta]$$

$$= qw'J'/4ch . \int_{-\pi/2}^{+\pi/2} d\theta[(3-\lambda)\cos\theta - 3(1-\lambda)\sin^2\theta\cos\theta]$$

$= J'qw'/ch$ dynes per cm. length.

The Lorentz force is given by

$$-dR_y = -\frac{qq'}{r^2}\cos\theta\left[1 + \frac{1}{2}\frac{w'^2}{c^2} - \frac{3}{2}\frac{w'^2\sin^2\theta}{c^2}\right] + \frac{qq'}{r^2}\cos\theta$$

$$= -\frac{qq'w'^2}{2c^2r^2}\cos\theta\,(1 - 3\sin^2\theta).$$

Whence $R_y = 0$. That is, there is no force, according to Lorentz's theory, between the current and the charged wire. Hence Mazwell (ii. 482) happened to be correct as regards this special case. But on Ritz's theory a small force exists, but it would be difficult to measure.[14] Taking $w'/c = 10^{-10}$, $J' = 100$ elm, $q = 10$ elst/cm., $h = 1$ cm., the force would be only 10^{-7} dyne/cm.

Referring to Fig. 57, we can find the force exerted by a circular coil (of radius a) on a charge e distant b from the centre in the plane of the coil. According to Lorentz it is

$$R = J'ew'a/2c . \int r^{-2}d\theta\cos(\theta + \varphi)(1 - 3\sin^2\varphi)$$

$$= \frac{J'ew'}{abc}\left[\frac{a^2+b^2}{a+b}K - (a+b)E\right].$$

Hence the force is not zero in this case.

According to Ritz the force is

$$F = J'w'a/4c . \int r^{-2}d\theta[\cos(\theta+\varphi)\{3 - \lambda - 3(1-\lambda)\sin^2\varphi\} + (1+\lambda)\sin\theta\sin\varphi]$$

$$= J'bw'/ac . [K/(a+b) + E/(a-b)].$$

[14] Bush, p. 142.

11. Rotating Conductors.

Consider a magnet or system of closed currents symmetrical round the axis Oz. Let P be a point in a symmetrical conductor rotating uniformly with ω round Oz. The angular velocity is the vector $(0, 0, \omega)$, and $\mathbf{v}$ the velocity of P is $\omega r\,(-\sin\theta, \cos\theta, 0)$. Owing to the symmetry, A_1, A_2, A_3, the $r\theta z$ components of the vector potential are independent of θ. The xyz components are

$$A_x = A_1\cos\theta - A_2\sin\theta,\quad A_y = A_1\sin\theta + A_2\cos\theta,\quad A_z = 0.$$

Hence

$$\begin{aligned}(\mathbf{v}\nabla)\mathbf{A} &= \omega r\left(-\sin\theta\frac{\partial}{\partial x} + \cos\theta\frac{\partial}{\partial y}\right)\mathbf{A}\\ &= \omega\left(-y\frac{\partial}{\partial x} + x\frac{\partial}{\partial y}\right)\mathbf{A}\\ &= \omega\frac{\partial\mathbf{A}}{\partial\theta}.\end{aligned}$$

Or

$$\begin{aligned}(\mathbf{v}\nabla)A_x &= \omega\partial A_x/\partial\theta = -\omega A_y\\ (\mathbf{v}\nabla)A_y &= \omega A_x,\quad (\mathbf{v}\nabla)A_z = 0.\end{aligned}$$

Therefore, since $V\boldsymbol{\omega}A = \omega(-A_y, A_x, 0)$, we have $(\mathbf{v}\nabla)\mathbf{A} = V\boldsymbol{\omega}\mathbf{A}$. Since $\partial\mathbf{A}/\partial t = 0$, we might have written this down at once, each side being equal to $d\mathbf{A}/dt$. It follows that

$$\begin{aligned}\nabla(\mathbf{vA}) &= V\mathbf{v}\,\mathrm{curl}\,\mathbf{A} + (\mathbf{v}\nabla)\mathbf{A} - V\boldsymbol{\omega}\mathbf{A}\\ &= V\mathbf{v}\,\mathrm{curl}\,\mathbf{A}\end{aligned}$$

so that $c^{-1}V\mathbf{vH} = \nabla\chi$, where χ is $(\mathbf{vA})/c$. (12.35)

Consider a magnet magnetised along the z axis (Fig. 59).

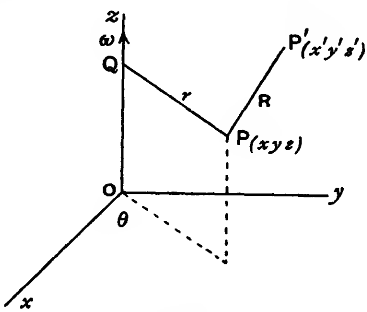

Fig. 59.

$$\mathbf{A} = \int d\tau' VI\nabla'\frac{1}{R},$$

$$A_x = -I\int\frac{\partial}{\partial y'}\frac{d\tau'}{R} = I\frac{\partial}{\partial y}\int\frac{d\tau'}{R},$$

$$A_y = I\int\frac{\partial}{\partial x'}\frac{d\tau'}{R} = -I\frac{\partial}{\partial x}\int\frac{d\tau'}{R},$$

$$A_z = 0.$$

Hence

$$\chi = \omega r/c \,.\, (-A_x \sin\theta + A_y \cos\theta)$$
$$= -\frac{\omega r I}{c} \cdot \frac{\partial}{\partial r}\int \frac{d\tau'}{R}$$
$$= \omega r/c \,.\, IF, \tag{12.36}$$

where F is the attraction at P along PQ due to a body of unit density. For example, suppose we have a thin magnet ns (Fig. 60) with pole-strength $m = IS$ where S is the cross-section.

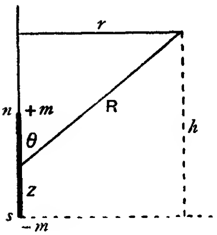

Fig. 60.

Differentiating $h - z = r \cot\theta$, we have $R^2 d\theta = r dz$. The attraction of a rod of unit density, or mass S per unit length, is

$$F = \int S dz/R^2 \,.\, \sin\theta = S/r \,.\, \int_s^n \sin\theta d\theta$$
$$= S/r \,.\, (\cos\theta_s - \cos\theta_n).$$

Hence

$$\chi = \omega m/c \,.\, (\cos\theta_s - \cos\theta_n)$$
$$= \omega m/2\pi c \,.\, (\Omega_n - \Omega_s), \tag{12.37}$$

where $\Omega_n = 2\pi(1 - \cos\theta_n)$ and $\Omega_s = 2\pi(1 - \cos\theta_s)$ are the solid angles subtended at n and s by the circle r.

We can express χ also as follows. Let the magnetic intensity be H_1 along r and H_3 along z. Then

$$V\mathbf{vH} = -V\mathbf{H}V\boldsymbol{\omega}\mathbf{r}$$
$$= -\boldsymbol{\omega}(\mathbf{rH}) + \mathbf{r}(\boldsymbol{\omega}\mathbf{H}),$$

i.e. $-\omega r H_1$ along z and $+\omega r H_3$ along r.

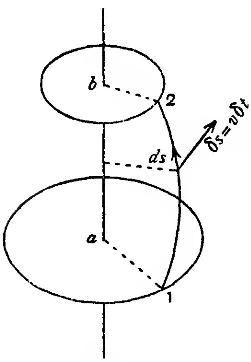

Fig. 61.

Hence

$$\chi = \omega/c \,.\, \int r(H_3 dr - H_1 dz). \tag{12.38}$$

For example, in the case of a uniform field parallel to the axis of rotation, $\chi = \omega H/2c \,.\, r^2 +$ constant.

Suppose we have a curve 12 rotating about the axis of symmetry (Fig. 61). The outward directed element of area described by ds in time δt is $V\delta\mathbf{s}\mathbf{ds} = \delta t V\mathbf{v}\mathbf{ds}$. The outward flux through this element of described area is

$$\delta t(\mathbf{H}V\mathbf{v}\mathbf{ds}) = -\delta t\,(\mathbf{ds}V\mathbf{vH}).$$

Hence, if $\delta N'$ is the flux cut through in time δt by the curve 12,

$$\begin{aligned}\chi_2 - \chi_1 &= c^{-1}\int_1^2 (\mathbf{ds}\,V\mathbf{vH}) \\ &= -\,c^{-1}\delta N'/\delta t \\ &= -\,c^{-1}N'/T \\ &= -\,\omega/2\pi c\,.\,N',\end{aligned}$$

where N' is the flux swept through by 12 in one complete revolution. But if N_1 and N_2 are the positive (upward) fluxes swept through by the radii of the circles 1 and 2, $N' - N_1 + N_2 = 0$, since the total outward magnetic flux is zero. That is

$$\chi_2 - \chi_1 = \omega/2\pi c\,.\,(N_2 - N_1). \tag{12.39}$$

If a symmetrical conductor is rotating in a symmetrical magnetic field (i.e. s' at rest), the intensity at any point is, as we have seen, $\mathbf{F} = c^{-1}V\mathbf{vH} = \nabla\chi$. If the electricity in the conductor is to be in relative equilibrium, this force must be balanced by a distribution of free electricity, i.e. one producing a force $-\nabla\chi$ or an electrostatic potential $\varphi = \chi + \text{constant}$.[15] As will be apparent from our remarks on Budde, this holds only for the first order in v/c.[16]

Suppose we have a conducting sphere in a uniform field H, rotating about the diameter in the direction of H. We have

$$\begin{aligned}\varphi &= \omega H/2c\,.\,r^2 + C \\ &= \omega H/2c\,.\,R^2\sin^2\theta + C,\end{aligned}$$

where R is the distance from the centre, making θ with the axis. Hence $\rho = -\nabla^2\varphi/4\pi = -\omega H/2\pi c$. We can put φ in the form

$$\varphi = C + \omega Ha^2/3c\,.\,(R^2/a^2) - \omega Ha^2/3c\,.\,P_2(R^2/a^2),$$

where a is the radius of the sphere and $P_2 = (3\cos^2\theta - 1)/2$. Hence the potential at outside points is

$$\varphi' = (C + \omega Ha^2/3c)(a/R) - (\omega Ha^2/3c)P_2(a/R)^3.$$

[15] Jochmann (i. 508) wrote in 1864: 'A distribution of free electricity within and upon the surface of the conductor can always be assigned so that its potential at every point of the conductor equilibrates the e.m.f. induced by the magnetic field and thus prevents the production of currents.' Larmor in 1884 (ii. 18) said that 'the true value of χ is that derived from axes fixed with reference to some system or medium which is the seat of the electromagnetic action.' But we have proved that the formula $\mathbf{F} = \nabla\chi$ holds also in Ritz's theory.

[16] Larmor (ii. 18): 'This static charge itself exerts a magnetic effect by virtue of its motion; but it is easy to see that this depends on v^2 and is therefore very minute.'

The surface density is

$$\sigma = -\frac{1}{4\pi}\left(\frac{\partial\varphi'}{\partial R} - \frac{\partial\varphi}{\partial R}\right)_{R=a}$$

$$= C/4\pi a + \omega Ha/24\pi c\,.\,(11 - 15\cos^2\theta).$$

And the total quantity of free electricity on the sphere is

$$e = 4\pi a^3\rho/3 + \int\sigma dS$$

$$= \omega Ha^3/3c + Ca.$$

This can be seen otherwise; for, since the P_2 term in φ' can give no charge on integration, the first term must be the potential of the charge. Hence when the sphere is insulated $C = -wHa^2/3c$. When the axis is earthed, $\varphi = 0$ when $\theta = 0$, therefore $C = 0$.

If the sphere is magnetic, we must put curl $\mathbf{A} = \mathbf{B}$ instead of $\mathbf{H}$. Suppose we have an iron sphere $S_1(a)$ surrounded by a concentric metal shield $S_2(b)$. Initially the rotating sphere S_1 is insulated, so that the potential at a point on the shield is [17]

$$\varphi' = (\omega Ba^2/3c)P_2(a/b)^3.$$

If the axis is now earthed, the potential becomes

$$(\omega Ba^2/3c)(a/b) + \varphi',$$

so that the change in the potential is $\omega Ba^3/3bc$. This was experimentally verified by Swann (ii. 38).

Let us next consider another type of system (Fig. 62). M is

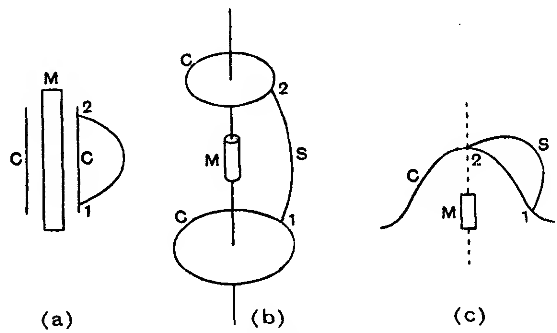

Fig. 62.

a symmetrical magnet which can rotate about its axis. C is an insulated symmetrical shield—cylindrical as in (*a*), circular as in

[17] In deriving the external potential from the internal we must take B to be constant, i.e. it still denotes the constant magnetic induction through the sphere.

(*b*), or bell-shaped as in (*c*)—which can be made to rotate with M. S is a wire with sliding contacts, 1 and 2, on C. The force exerted by a neutral closed circuit on a charge e moving with $\mathbf{v}$ is, since $V\boldsymbol{\omega}\mathbf{A} = \mathbf{d}\mathbf{A}/dt$,

$$\mathbf{F} = \nabla(\chi + \psi). \tag{12.40}$$

So we have the three cases :

(1) Circuit at rest, e moving round it : $\mathbf{F}_1 = \nabla\chi$.
(2) Circuit rotating, e at rest : $\mathbf{F}_2 = \nabla\psi$.
(3) Circuit rotating, e moving round it : $\mathbf{F}_3 = \nabla(\chi + \psi)$.

For the present we will not take into account any readjustment due to the metallic connection of the points 1 and 2. We can take the shunt S merely as two wires connected to the quadrants of an electrometer ; or, what is practically equivalent, the points may be connected through a galvanometer and a sufficiently high resistance.

Suppose that C alone rotates. This is the case of a conductor moving in a stationary magnetic field, which has been previously investigated. We have $\mathbf{F}_1 = \nabla\chi$; and, as already explained, this is balanced by $\mathbf{F}_1' = -\nabla\chi$, so that the static potential is $\varphi = \chi +$ constant. Hence $\varphi_2 - \varphi_1 = \chi_2 - \chi_1$.

Suppose that M and C rotate together, not necessarily at the same rate. Then $\mathbf{F}_3 = \nabla(\chi + \psi)$. If there is relative equilibrium of electricity, this is balanced by an electrostatic intensity of potential $\varphi = \chi + \psi +$ constant, so that

$$\varphi_2 - \varphi_1 = (\chi_2 + \psi_2) - (\chi_1 + \psi_1).$$

But in the stationary system S there is an intensity $\mathbf{F}_2 = \nabla\psi$, producing a potential difference $\psi_1 - \psi_2$. Thus the total potential difference is $\chi_2 - \chi_1$ as before.

Suppose that M alone rotates. In C we have, similarly to the preceding cases, $\varphi_2 - \varphi_1 = \psi_2 - \psi_1$; and in S there is a potential difference $\psi_1 - \psi_2$. Hence the net p.d. is zero.

If S alone rotates, $\mathbf{F}_1 = \nabla\chi$. Therefore $\varphi_2 - \varphi_1 = \chi_1 - \chi_2$, i.e. minus the e.m.f. for the case of M and C rotating together. Hence if S and C rotate, M being at rest, the e.m.f. is zero.

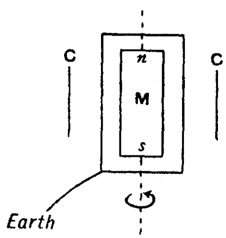

Fig. 63.

Fig. 63 illustrates the essentials of experiments by Kennard, Barnett, and Pegram. M is the magnet surrounded by an earthed metal case, C is an outer insulated cylindrical

case. Suppose M is at rest and C rotating. Then the potential at any point of C is $\varphi = \chi + \text{constant} = \omega N/2\pi c + \text{constant}$. If M is also rotating the potential would seem to be $\varphi = \chi + \psi + \text{constant}$. But, as we have seen in connection with a rotating coil, the potential ψ is the same as that produced by a charge-distribution on the magnet. It is therefore screened from the condenser by the earthed shield surrounding the magnet. Hence the potential is $\varphi = \chi + \text{constant}$ as when the magnet is not rotating. These experiments have been so misinterpreted that it is necessary to make the simple observation that they cannot discriminate between the theories of Lorentz and Ritz.

Suppose (Fig. 64) that the two circular circuits 1 and 2 are connected by the fixed wire P_1P_2 and the rotating wire M_1M_2, the currents and resistances (in elsts) being as marked in the figure. Denote the potential at any point by the letter referring to the point. Thus M_1 is the potential at the point M_1 of the fixed circuit 1. This is not the same as the potential M_1' at the point of the moving wire. Since $\varphi = \text{constant} - \omega/2\pi c \,.\, N$, there is a finite potential-difference given by $M_1' = M_1 - \omega/2\pi c \,.\, N_1$; and similarly $M_2' = M_2 - \omega/2\pi c \,.\, N_2$. We have

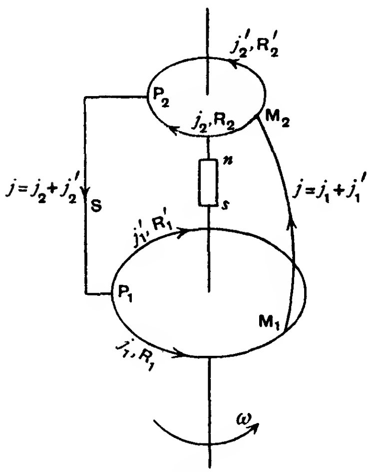

Fig. 64.

$$P_1 - M_1 = j_1R_1 = j_1'R_1',$$
$$M_2 - P_2 = j_2R_2 = j_2'R_2'.$$

Hence

$$P_1 - M_1 = jX_1, \quad M_2 - P_2 = jX_2,$$

where $X_1 = R_1R_1'/(R_1 + R_1'), \quad X_2 = R_2R_2'/(R_2 + R_2').$

Now

$$jS = P_2 - P_1$$

and

$$jR = M_1' - M_2' = M_1 - M_2 + \omega/2\pi c \,.\, (N_2 - N_1).$$

Hence [18]

$$j(R + S + X_1 + X_2) = \omega/2\pi c \,.\, (N_2 - N_1).$$

[18] We have assumed of course that the current is so feeble that its induction is negligible relatively to that of the magnet.

We may in practice neglect $X_1 + X_2$ in comparison with $R + S$, so that

$$j = \omega(N_2 - N_1)/2\pi c(R + S). \tag{12.41}$$

As we have referred above to the problem of a sliding contact, we shall add a few remarks. As in Fig. 64 take the inducing circuit c' (or magnet) to be at rest, and let the induced circuit c have a moving portion. From (12.35) the e.m.f. induced in ds is given by

$$\delta V dt = -\delta\left[ds \int j'ds' \cos \varepsilon/cr\right].$$

Suppose that, between the instants t and $t + dt$, the element ds is introduced into the circuit c, so that $\delta ds = ds = vdt$. Then, the variation in the integral being zero from reasons of symmetry,

$$\delta V = -v \int j'ds' \cos \varepsilon/cr,$$

i.e. is finite. But the current is not infinite, for the e.m.f. is compensated by p.d. due to charges. Suppose the sliding contact is at A, B, C at times $t - dt$, t and $t + dt$. Using A to denote the potential at A and so on, we have $C - B + \delta V = 0$. Similarly $ds = AB$ has been eliminated from the circuit, so that $\delta ds = -ds$. Hence $B - A - \delta V = 0$. That is, $A = C$. On the other hand, if ds is *not* one of the elements of c which are swept out by the sliding contacts between t and $t + dt$, we have $\delta ds = 0$, since the element is rigid. Hence in this case, $\delta V = 0$, so that the potential remains constant along each element of c; or $A = C =$ constant. Using $\varphi \equiv B$ for the potential of the moving contact, we have

$$\varphi = C + \delta V.$$

Or, using mean values,

$$\begin{aligned}\varphi_m &= \frac{1}{T}\int \varphi dt \\ &= C - \frac{1}{T}\int\int ds ds' j' \cos \varepsilon/cr \\ &= C - \omega/2\pi c \,.\, N,\end{aligned}$$

where N is the flux through the symmetrical circuit (1 or 2 in Fig. 64). This is the formula which we used above. Were the system perfectly symmetrical, the mean value would be the actual value.

But the argument leading to (12.41) applies only to the mean

value of the current, for the presence of the wave P_1P_2 disturbs the symmetry and its effect can be eliminated only by taking a complete number of periods. We can avoid considering the p.d. at the sliding contacts (wires dipping into circular troughs of mercury) by using (12.39) :

$$V = \varphi_1 - \varphi_2 = X_2 - X_1$$
$$= \omega/2\pi c \,.\, (N_2 - N_1).$$

Whence the current in elsts (taken positively upwards) is

$$j = V/(R + S) = \omega(N_2 - N_1)/2\pi c(R + S).$$

Since there is no impressed e.m.f., we have

$$j^2(R + S) + G\omega = 0,$$

where G is the torque acting on 12. That is,

$$G = -j/2\pi c \,.\, (N_2 - N_1) = -jV/\omega.$$

Now G is necessarily negative, i.e. it is a resisting torque. Hence j is positive (upwards) only when $N_2 > N_1$. The positive torque *against* which the wire 12 is rotated is given by $j(\varphi^1 - \varphi_2)/\omega$ when j is taken positively upwards from 1 to 2, and $j(\varphi_2 - \varphi_1)/\omega$ when j is taken downwards—a formula which we shall meet again in (12.42).

If $N_2 - N_1$ is not zero, a current will flow. If the circuit is completed through the magnet itself as in Fig. 65, $N_2 = 0$ so that j is negative, i.e. the current is as marked in the figure, provided the n-pole is uppermost and the rotation of the wire is anti-clockwise. If it is the *magnet* that it is rotating, the current is in the opposite direction, since in this case the formula for M and C rotating is $\varphi_2 - \varphi_1 = \chi_2 - \chi_1$.

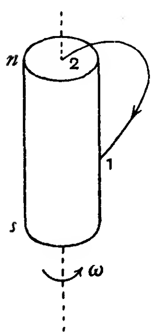

Fig. 65.

Cramp and Norgrove (p. 488) performed the experiment illustrated in Fig. 65, the magnet (not the circuit) being rotated, and 1 being a copper disc fixed on the equator of the magnet. The contact at 1 was made by means of a gauze brush, and the e.m.f. between 1 and 2 was measured by a galvanometer. Another experiment was then performed, in which the disc and sliding contact were not used ; the wire was connected to the equator of the magnet and so arranged that, as the magnet rotated, the wire was wound up into a coil (of the same mean radius as the disc) at the equator.

For the same speed of rotation, the e.m.f. was found to be the same. So the authors conclude (p. 489) that this

> shows that when tubes of induction are linked without being cut, no e.m.f. results. It would seem therefrom that, as Faraday supposed, the 'flux-cutting' rather than the 'flux-linking' law is the more fundamental.

But surely, without getting involved in metaphorical subtleties, the identity of the two cases is obvious. We have

$$\varphi_2 - \varphi_1 = \chi_2 - \chi_1 = \omega/2\pi c \, . \, (N_2 - N_1).$$

The increase of flux per revolution (N_1) is the same in both cases. In the one case this is effected by a sliding contact—which, the authors seem to forget, raises certain difficulties considered above; in the other case, the flux is changed by coiling the wire round the magnet.

Suppose we are dealing with a long thin magnet and that the circles 1 and 2 are small. As already proved (12.37),

$$V = \varphi_1 - \varphi_2 = \chi_2 - \chi_1$$
$$= \omega m/c \, . \, [\cos n1 - \cos n2 + \cos s2 - \cos s1],$$

where $n1$ is the angle subtended positively (upwards) by the radius of the circle 1. We have the four cases illustrated in Fig. 66. The potential V is zero in the first three cases:

(*a*) ns between the planes 1 and 2: $n1 = s1 = \pi$, $n2 = s2 = 0$.

(*b*) ns overlapping the planes: $n1 = n2 = \pi$, $s1 = s2 = 0$.

(*c*) ns outside 12: $n1 = n2 = s1 = s2 = 0$.

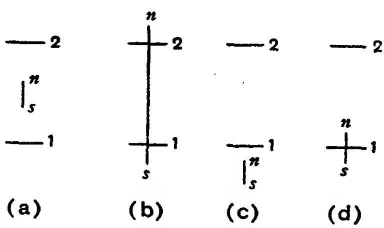

Fig. 66.

But in

(*d*) ns intersecting the plane 1: $n1 = \pi$, $n2 = s1 = s2 = 0$.

Therefore[19] $V = -2\omega m/c$.

Clearly when the circles 1 and 2 rotate and S is at rest, the results are the same, except that in case (*d*) the sign of V is

[19] The current is from the higher to the lower circle provided the rotation is positive (left to right), the n-pole is directed upwards, the magnet or solenoid crosses the plane of the lower circle. The direction of the current is reversed if one of these conditions is reversed.

reversed. Hence when the circles are small the current has a sensible value only when one pole is within their planes and the other is outside. This result has given rise to the very misleading term 'unipolar induction' which is applied to these phenomena.

The statement has been made that 'the theory of unipolar induction presents no difficulty if we adhere to the usual laws of induction.'[20] This assertion is worth investigating, for it is desirable to get rid of some widespread ambiguities. In the section on Induction we showed that the e.m.f. over a closed circuit is

$$V = \oint(\mathbf{F}d\mathbf{s}) = -\frac{1}{c}\frac{d}{dt}\oint(\mathbf{A}d\mathbf{s})$$
$$= -\frac{1}{c}\frac{d}{dt}\int(\mathbf{H}d\mathbf{S})$$
$$= -\frac{1}{c}\frac{dN}{dt}.$$

This formula is derived from the term $-c^{-1}d\mathbf{A}/dt$ in the expression for $\mathbf{F}$. And the argument essentially implies a *closed* circuit, for otherwise the gradient terms would not vanish nor could we apply Stokes's theorem to the circuit. Here S is any closed surface bounded by the circuit s; and N is the flux through this surface, dN/dt being the total rate of change of this flux, due both to the motion of the inducent s' and to the motion of the induced s. In the case now under consideration s' is at rest so that $\mathbf{F} = c^{-1}V\mathbf{v}\mathbf{H}$ and

$$\oint(\mathbf{F}d\mathbf{s}) = -\frac{1}{c}\frac{\delta N}{\delta t} = \frac{1}{c}\frac{\delta N'}{\delta t}.$$

And when the closed circuit rotates round the axis of symmetry, the integral is zero; for the flux N through it is constant and the rate of flux cut through is zero.

Now the essential formula for this case of so-called unipolar induction is

$$\varphi_2 - \varphi_1 = \chi_1 - \chi_2 = \int_2^1(\mathbf{F}d\mathbf{s})$$
$$= \frac{1}{c}\frac{\delta N'_{21}}{\delta t} = \frac{\omega}{2\pi c}N'_{21},$$

where N'_{21} means the flux cut through in one revolution by the incomplete circuit 21. Except by some vague considerations of

[20] Wiedemann, iv. 126.

symmetry, it is not easy to see how this could be deduced from the zero value of the integral over a closed circuit.[21] The attempt to deduce the unipolar formula from the induction-law for complete circuits must therefore be rejected.[22] It is the converse argument which is valid and easy. We have $\mathbf{F} = \nabla\chi$ since $\delta\mathbf{A}/\delta t = V\boldsymbol{\omega}\mathbf{A}$ by symmetry. Therefore

$$\int_2^1 (\mathbf{Fds}) = \chi_1 - \chi_2$$

and

$$\oint (\mathbf{Fds}) = 0.$$

Some general observations may now be made on so-called unipolar induction, concerning the explanation of which violent and useless controversies still rage. Prof. Swann wrote in 1920 (i. 367):

> A perusal of the literature on this subject suggests that many physicists have a feeling that there is a fundamental element of uncertainty as to what should happen in experiments of this kind; that there is in fact a question to which our electromagnetic scheme has no answer.

Similarly Prof. Tate wrote in 1922 (p. 75):

> The problem of unipolar induction has been correctly to account for the existence of this electric field. There seems always to have been an element of mystery connected with the phenomenon, and an element of doubt on the part of many physicists as to the power of electromagnetic theory to solve the problem uniquely.

According to our analysis the difficulty arises solely from the fact that physicists have neither logically developed Lorentz's electron theory nor taken the trouble to investigate Ritz's alternative theory. Naturally the followers of Einstein have seized the opportunity of fishing in troubled waters. 'The safest way to proceed to the result,' concludes Tate (p. 78), 'is to give up the semblance of predicting the field of the moving whirl, and

[21] Moullin (p. 65) says rather tragically that, whereas we know $\oint(\mathbf{Fds})$, 'no simple device has yet been found for calculating $\mathbf{F}$, this still remains outside human knowledge.' Yet he upholds Lorentz's theory!

[22] For example, Becker, p. 335. He asks us to 'apply the law of induction to the material integration-path.' i.e. to the circuit $a12ba$ (Fig. 61), which may be purely imaginary. He professes to derive a formula for $\delta N'/\delta t$ which he does not recognise as zero because he omits to take account of the radial portion of the 'path.' Then, without further ado, he equates this unproved result to $\phi_2 - \phi_1$.

instead to assume the field suggested by the theory of relativity.' 'The facts of unipolar induction,' says Pegram (p. 600), 'are in accord with the theory of relativity.' As we have already pointed out, Einstein's theory has nothing whatever to do with these results, to explain which he adopts the theory of an earth-convected aether.

The issue is therefore between the formulae of Lorentz and Ritz. These differ only in the respective values they give to ψ, which are not determined by any of the experiments, except that of M. and H. A. Wilson. With this exception both theories account for these phenomena; with the usual proviso that the aether in Lorentz's theory must be assumed to be convected with the laboratory. In Ritz's theory the laboratory system is any Newtonian system of reference, in Lorentz's formula it is the aether. It is therefore incorrect to say with Kennard (iii. 179) that 'the fundamental problem of unipolar induction is this: whether the induced e.m.f. is determined by the absolute rotation of the system or by the rotation of its parts relative to each other.' For acceleration, and therefore rotation, is absolute on Newtonian as well as on Lorentzian principles. Besides, if we take the electron theory seriously—which, in spite of the lip-service given to it, is rarely done—we cannot talk of a magnet or circuit as 'parts.' The formulae must ultimately be derived from the force-law between electrons; and if the angular velocity enters into it, it is only because electrons are moving with the rigid bodies (*e.g.* $v = \omega r$ in χ) or because their acceleration is involved (e.g. g'_r in ψ). It is only in this sense that we can admit Kennard's dictum (i. 941) that 'electromagnetic induction caused by motion depends on absolute motion.' And we must reject the following conclusion which he draws (iii. 190):

> This phenomenon seems to lend definite support to the existence of an electromagnetic aether. It is perhaps the only low-frequency phenomenon which cannot easily be described in terms of action-at-a-distance between electrons and atoms.

The incorrectness of this statement is made obvious by our preceding analysis, in which we showed that all the results are explained by Ritz's theory which does not assume such a medium. One might as well point to the tides or to Foucault's pendulum as lending definite support to the existence of an aether! Nor is there anything unique about the phenomenon; it is explained

by the second-order force-law (involving only simultaneous quantities) just as were Ampère's current-formulae.

It would be a waste of time to resuscitate the controversy as to whether the force exerted by a current element P on a magnetic pole M was applied at M (as Biot held) or at a point coinciding with P but rigidly connected with M (as Ampère held). According to the electron theory the force exerted by the charges in a circuit acts on the charges in the element of the other circuit and indirectly on the material conductor. But the futile and barren controversy as to whether the 'lines of force' of a circuit move with the circuit or remain stationary in the alleged aether, still encumbers the literature of physics. So prone is the human mind to hypostatise its own metaphors! Faraday (iii. 336, § 3090) wrote in 1852:

> When lines of force are spoken of as crossing a conducting circuit, it must be considered as effected by the *translation* of a magnet. No mere rotation of a bar magnet on its axis produces any induction effect on circuits exterior to it. . . . The system of power about the magnet must not be considered as necessarily revolving with the magnet, any more than the rays of light which emanate from the sun are supposed to revolve with the sun. The magnet may even in certain cases be considered as revolving amongst its own forces.

This theory of stationary lines is nowadays the most widely accepted. But it is merely an out-of-date invention adopted for those who are supposed not to be able to grasp the mathematical idea of a vector field. Once more, if we take the electron theory seriously, we must start with the formula which gives the resultant force due to the moving charges constituting the magnet, namely,

$$\begin{aligned}\mathbf{F} &= \nabla(\chi + \psi) + c^{-1}V\boldsymbol{\omega}\mathbf{A} - c^{-1}\mathbf{dA}/dt \\ &= \nabla\psi + c^{-1}V\mathbf{vH} - c^{-1}\partial\mathbf{A}/\partial t.\end{aligned}$$

Here the $\boldsymbol{\omega}$ or the $\mathbf{v}$ is certainly not relative to the rotating magnet. In *this* sense the theory of stationary lines must be accepted. On the other hand the force is not the same as if the magnet were stationary ($\mathbf{F}_0 = c^{-1}V\mathbf{vH}$).

But once we begin talking of 'magnetic lines of force,' we are assuming two complete neutral circuits, the force between which is independent of their velocities. The auxiliary vector $\mathbf{H} = \operatorname{curl} \mathbf{A}$ remains the same at every point of space round a rotating symmetrical magnet. This is all that is expressed by saying that the field is stationary.

12. The Torque on a Rotating Magnet.

The problem of the torque on a rotating magnet will now be investigated. Let us first find the torque *against* which the wire 21 is rotated (Fig. 67). Suppose a current, J elm or j elst, is flowing from 2 to 1, and let there be a pole $+m$ at the origin. The force on an element ds is $\mathbf{dF} = JV\mathbf{dsH}$. Hence it is only the component of $\mathbf{ds}$ in the meridian plane that counts; accordingly

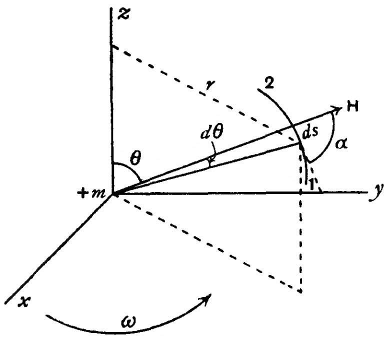

Fig. 67.

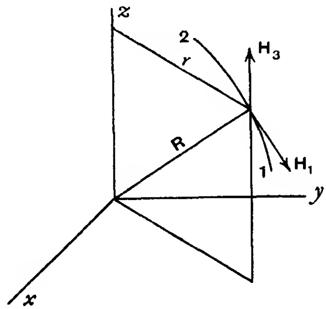

Fig. 68.

we can without loss of generality take 12 as a curve in the meridian plane. Since $H = m/R^2$ along R, we have $dF = mJds \sin \alpha \, . \, /R^2$ in the negative direction, i.e. contrary to that of the angular velocity ω. Since $r = R \sin \theta$ and $ds \sin \alpha = Rd\theta$, the resisting couple is

$$dG = rdF = mJ \sin \theta d\theta,$$

or, for the whole wire,

$$G = mJ \int_2^1 \sin \theta d\theta = -mJ[\cos (n1) - \cos (n2)].$$

Taking the negative pole into account,

$$G = -mj/c \, . \, [\cos (n1) - \cos (n2) + \cos (s2) - \cos (s1)]$$
$$= -j/\omega \, . \, (\varphi_1 - \varphi_2),$$

from our previous result. That is,

$$G\omega = j(\varphi_2 - \varphi_1). \tag{12.42}$$

As we should expect, the work expended in producing the current is equal to the work done against the resisting torque.

More generally we can proceed as follows (Fig. 68), taking the

current from 1 to 2 for convenience. The force on an element **ds** is $JV\mathbf{ds}\mathbf{H}$ and the torque is

$$JV\mathbf{R}V\mathbf{ds}\mathbf{H} = J\mathbf{ds}(\mathbf{HR}) - J\mathbf{H}(\mathbf{Rds})$$
$$= J\mathbf{ds}(H_1r + H_3z) - J\mathbf{H}(rdr + zdz).$$

The z-component of this is

$$dG = Jdz(H_1r + H_3z) - JH_3(rdr + zdz)$$
$$= -Jr(H_3dr - H_1dz)$$

Therefore the torque acting *on* 12 is

$$G = -j/c \,.\int_1^2 r(H_3dr - H_1dz)$$
$$= -j/\omega \,.\, (\chi_2 - \chi_1).$$

That is

$$G\omega = -j(\varphi_1 - \varphi_2).$$

We can also express the torque as

$$G = J/2\pi \,.\, (N_1 - N_2). \tag{12.43}$$

Suppose (Fig. 69) that a current is passed through a conducting magnet capable of rotation, the current entering through an attached arm or apron so that there are sliding contacts at 1 and 2. Taking 241 to be the C of our previous notation, and 132 (the circuit in the magnet M) to be the S, we have C at rest while S and M rotate, being acted upon by a torque [23] $G = J/2\pi \,.\, (N_1 - N_2)$. Thus the torque on the magnet may be regarded as a torque exerted by the external circuit on the current passing through the material of the magnet. This is the interpretation of Pietenpol and Westerfield.

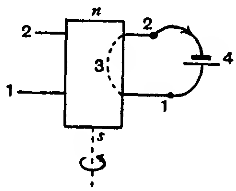

Fig. 69.

On the other hand we may regard 241 as fixed S and 132 as C rotating with M. In this case we have the formula $\varphi_1 - \varphi_2 = \chi_1 - \chi_2$ so that the back e.m.f. is

$$V' = \omega/2\pi c \,.\, (N_1 - N_2).$$

If V is the impressed e.m.f.,

$$jR = V - V',$$

[23] S is not a linear circuit, but the same result clearly holds for a number of such circuits and therefore for a current-sheet. Also no error is involved in treating the current in the magnet as if it were in the plane through ns, since the perpendicular component contributes nothing to the torque.

and if G is the torque on the magnet, the equation of activity is

$$jV = j^2R + G\omega.$$

Whence once more we obtain

$$G = j/2\pi c \,.\, (N_1 - N_2).$$

Or otherwise : when S alone is rotating the couple on it is

$$G = -\, J/2\pi \,.\, (N_1 - N_2),$$

the minus occurring because J is now from 2 to 1 through S. It is clear from our previous reasoning (or from the fact that there is no couple when M, C and S all rotate) that when M and C rotate the couple on them is $-\, G = J/2\pi \,.\, (N_1 - N_2)$. This is the view taken by Kimball (p. 1302) : 'The torque on the magnet comes entirely from the reaction between the flux of the magnet [supposed to be carried round by M] and the fixed external circuit which is not carried by the magnet.' Which is merely an involved way for saying that the couple of the external circuit on M is equal and opposite to the torque of M on the circuit.

The view of Zeleny and Page (i. 549) is : 'The total torque is that exerted by the field of the magnet on the current passing through the magnet and through any apron or arm which may be attached thereto.' At this stage the metaphor of 'the field' becomes very misleading. If it is seriously contended that the action between current-elements and Amperian circuits, both within the same solid body, can produce rotation of the body as a whole, the assertion must be rejected as mechanically untenable.

13. A Moving Magnet.

According to Ritz a linearly moving magnet exerts on a stationary charge a force

$$\mathbf{F} = -\, c^{-1} V\mathbf{v}\mathbf{H},$$

which is obtained from (10.24b) on replacing v' by v. Now

$$\mathbf{dA}/dt = \partial\mathbf{A}/\partial t + (\mathbf{v}\nabla)\mathbf{A} = 0.$$

Hence

$$\begin{aligned}\mathbf{F} &= -\, c^{-1} V\mathbf{v} \text{ curl } \mathbf{A}\\ &= -\, \nabla\chi - c^{-1}\partial\mathbf{A}/\partial t.\end{aligned}$$

The latter term (the rate of change at $\mathbf{A}$ at a fixed point) is zero in the case of a practically infinite homogeneous magnetic medium.

The vector potential, being a summation applied to all the microscopic circuits, is

$$\mathbf{A} = \int d\tau V \mathbf{I} \nabla \frac{1}{r}.$$

Hence

$$\begin{aligned}\chi &= c^{-1}\Sigma(\mathbf{v}\delta\mathbf{A}) \\ &= c^{-1}\int d\tau \left(\mathbf{v} V \mathbf{I} \nabla \frac{1}{r}\right) \\ &= c^{-1}\int d\tau \left(V\mathbf{v}\mathbf{I} \,.\, \nabla \frac{1}{r}\right) \\ &= \int d\tau \left(\mathbf{Q} \nabla \frac{1}{r}\right),\end{aligned}$$

where $\mathbf{Q}$ is $c^{-1}V\mathbf{v}\mathbf{I}$. That is, χ is the scalar potential of an electrically polarised body with polarisation $\mathbf{Q}$. We can therefore say that the moving magnet is equivalent to an electrically polarised body [24] :

$$\mathbf{F} = -\nabla \int d\tau \left(\mathbf{Q} \nabla \frac{1}{r}\right) \tag{12.44}$$

But this is only on Ritz's theory. According to Lorentz

$$\begin{aligned}\mathbf{R} &= \nabla\chi - c^{-1}V\mathbf{v}\mathbf{H} \\ &= c^{-1}(v\nabla)\mathbf{A} \\ &= -c^{-1}\partial\mathbf{A}/\partial t,\end{aligned}$$

which, on the same supposition as previously made, is zero. According to Lorentz also the stationary charge exerts no force on the moving magnet.

Now, curiously enough, upholders of Lorentz's theory wish to adopt Ritz's result without his theory. This is how they do it :

> The mere fact of the absence of resultant force upon a charge which accompanies a magnet in uniform rectilinear motion requires a rearrangement of electric density in the Amperian whirls or their equivalents which constitute the molecular magnets; and this rearrangement is such as to endow the magnetic doublets with the properties of electric doublets as well.—Swann, i. 365.

[24] Or we might argue thus. Inside the body we take $\mathbf{F} = -c^{-1}V\mathbf{v}\mathbf{B}$. Hence, since curl $\mathbf{H} = 0$,

$$\begin{aligned}c \operatorname{div} \mathbf{F} &= (\mathbf{v} \operatorname{curl} \mathbf{B}) = 4\pi(\mathbf{v} \operatorname{curl} \mathbf{I}) \\ &= -4\pi \operatorname{div} V\mathbf{v}\mathbf{I}.\end{aligned}$$

Since there is no density of free electricity, div $\mathbf{F} = -4\pi$ div $\mathbf{Q}$, where $\mathbf{Q}$ is the polarisation. Therefore $\mathbf{Q} = c^{-1}V\mathbf{v}\mathbf{I}$.

The 'mere fact' however is nothing but Budde's hypothesis to save the stationary aether. The sum total of Swann's statement is simply the enwrapping in concrete physical language of the operation of boldly removing the $\nabla\chi$ from Lorentz's **R**. It can hardly be called a proof!

It should be clearly understood that the present difficulty has nothing to do with 'relativity,' for we are dealing exclusively with scientific experiments in a laboratory. The following contention may or may not be true; at least undergraduates are solemnly assured in a recent text-book that it is a fact:

> An observer travelling with a moving charge detects no magnetic field. In other words, a moving charge exerts no force or torque on a magnet which is moving along with it, but only on a magnet which does not partake of its motion.—Page-Adams, p. 240.

And presumably the magnet exerts no force on a comoving charge—*for a comoving observer.* As soon as this peculiar observer is produced, we can pass scientific judgement on the contention here made vicariously for him. Meanwhile we are concerned with the observer at rest in a laboratory. For him, on Lorentz's theory which is accepted by Einstein, the proposition is not true. And if, in order to make it true, we postulate 'a rearrangement'—depending of course on absolute motion—we must assign some cause not contained in Lorentz's theory.

Let us now examine the case of a symmetrical non-conducting soft magnet rotating in a uniform field along the axis. We have already seen that the force of the rotating magnet on a stationary unit charge is $\nabla\psi$. To find ψ we must divide the magnet into infinitesimal Amperian circuits, each of which may be taken as moving at any moment with the linear velocity $\mathbf{v}'$ of that point of the magnet. That is, the variations of the velocity along the micro-circuit are negligible, so that $\mathbf{g}' = 0$. Hence for Lorentz $\psi = 0$, and there is no force on the stationary charge.

Not so for Ritz, in whose formula for ψ there is a second term which —dropping the dashes—is

$$-\int Jv/cr \,.\, ds \cos (vds),$$

where r is the distance from ds to the point-charge. For any one of the little circuits the integral is simply $-(\mathbf{v}\delta\mathbf{A})/c$. Hence we have

$$\psi = -\int d\tau \left(\mathbf{Q}\nabla\frac{1}{r}\right)$$

and the force is $+\nabla\psi$. In other words, on Ritz's theory, the rotating magnet is equivalent to an electrically polarised body, in which the polarisation at any point is $\mathbf{Q}=c^{-1}V\mathbf{vI}$, and the force is given by (12.44).

Some observations on this proof will not be irrelevant. In the first place, the complete expression for the force on a stationary charge is

$$\nabla\psi - c^{-1}\partial\mathbf{A}/\partial t,$$

but in the present case the latter term is zero by symmetry. Next, when the magnet is a conductor, the effect is counteracted by a distribution of free electricity; that is, the force exerted by the charge + 1 at any point inside the conductor is neutralised. Also there is no justification for regarding the electric polarisation as really existing in a non-conductor, as something more than a mathematical equivalent of the force exerted by the moving sub-circuits.[25] And lastly the following statement is equally unjustified [26]:

> A circular current loop set in rotation about its axis would be surrounded by no electric field. A rotating magnetic shell on the other hand would be surrounded by an electric field since each elementary magnetic doublet of the shell would become in addition an electric doublet with axis radially out from the axis of rotation. There is thus in principle a fundamental difference between the unipolar induction effects of a rotating solenoid and those of a rotating material magnet. . . . There is an essential difference, not always realised, between unipolar induction experiments done with rotating solenoids and those done with rotating material magnets.

We have seen that a circular current rotating about its axis *is* in fact surrounded by an electric field. But in the case of a rotating material magnet we have a series of such microscopic currents, not rotating round their respective axes but each moving practically with a translational velocity. The two cases are entirely distinct; there is a difference in the application of the same formula, but no discrepancy in principle.

We can now consider the case of a rotating magnetic dielectric symmetrical round the axis along which the field acts. Whereas

[25] Barnett (vii. 1114) speaks of 'the charges developed by the motion.' But Tate (p. 94) refers to 'the fictitious polarisation in the dielectric produced by the motion of the magnetic doublets.'

[26] Tate, pp. 82, 92. Unipolar induction experiments are performed with *conducting* magnets.

in the case of a conductor we have $\varphi = \chi +$ constant so that $\mathbf{E} + c^{-1}V\mathbf{vB} = 0$, we now have an electric polarisation given by

$$4\pi\mathbf{P} = (\varkappa - 1)(\mathbf{E} + c^{-1}V\mathbf{vB}).$$

Or, since all the quantities are along r,

$$4\pi P = (\varkappa - 1)(- d\varphi/dr + \omega/c \,.\, Br).$$

In the experiments the dielectric was in the form of a hollow cylinder (internal radius a_2, external a_1) enclosed by brass tubes which formed metal coatings, the inner of which was earthed and the outer connected to an electrometer. Applying Gauss's theorem to a cylinder bounded by r and a_1 and of unit height, we have, assuming also a polarisation $\mathbf{Q} = c^{-1}V\mathbf{vI}$,

$$- (E + 4\pi P + 4\pi Q)2\pi r = 4\pi q,$$

where q is the charge per unit length on the inner face of the outside coating. Hence, substituting

$$4\pi Q = 4\pi/c \,.\, \omega r I = (1 - 1/\mu)vB/c,$$

we have

$$- 2q/\varkappa r = - d\varphi/dr + (1 - 1/\varkappa\mu)\omega Br/c.$$

On integration this becomes

$$- 2q/\varkappa \,.\, \log (a_1/a_2) = - (\varphi_1 - \varphi_2) + (1 - 1/\varkappa\mu)\omega B(a_1^2 - a_2^2)/2c.$$

On putting $\varkappa = \infty$, we obtain the previous case of a conductor.

This formula has been verified[27] for ordinary dielectrics for which practically $\mu = 1$. Only one experiment has been performed for a dielectric in which the permittivity could be taken as greater than unity. M. and H. A. Wilson made a composite dielectric by embedding small steel spheres in wax, for which μ could be taken as 3 for macroscopic volumes. This experiment confirmed the above formula. In describing their experiment the authors declare (pp. 99, 105):

> According to the theory based on the principle of relativity, this induced e.m.f. should be equal to that in a conductor multiplied by $(1 - 1/\varkappa\mu)$, . . . whereas, according to the theory of H. A. Lorentz and Larmor, the appropriate multiplier appears to be $(1 - 1/\varkappa)$ as for a non-magnetic insulator. . . . These experiments therefore confirm the theory of relativity, but do not necessarily conflict with the fundamental assumptions of H. A. Lorentz and Larmor's theory.

Lorentz himself wrote (xiii. 304): 'According to the older electron theory . . . the effect should be proportional to $\varkappa - 1$,

[27] R. Blondlot, CR 133 (1901) 778; H. A. Wilson, PT 204A (1904) 121; M. and H. A. Wilson, PRS 89A (1913) 99; S. J. Barnett, PR 27 (1908) 425; L. Slepian, AP 45 (1914) 861.

. . . according to the theory of relativity the effect should be proportional to $\kappa\mu - 1$.' This contention is still maintained.[28]

Prescinding from the question whether **Q** is experimentally required—a point which is undecided—let us examine some of the attempts to give a theoretical proof. That is, of course, apart from Ritz's theory which is never so much as mentioned. On the part of those who adhere to the Lorentz-Liénard theory there are some feeble attempts at a proof:

> The magnetisation I may be considered equivalent to a series of magnetic double layers. The motion of these double layers produces Q just as the electrical double layers give *J*.—H. A. Wilson, ii. 24*.
>
> The expression $\partial \mathrm{I}/\partial t$ represents the rate of change of the magnetic polarisation at a fixed point in the field only when the magnetic media as a whole are at rest. When these media are in motion there will be a contribution to this rate due to convection just as in the electric case, and the argument for its exact form may be developed on the same lines.—Livens, iii. 211.

But if we turn back to the proof of formula (10.38) for **J**, we shall at once see the worthlessness of this contention. It practically amounts to a complete repudiation of the electronic theory of magnetism, the substitution of magnetic poles therefor and the acceptance of 'magnetic currents.' Accepting the electron theory, we have just seen that in two cases—uniform linear motion and uniform rotation—the term **Q** does *not* occur on the Lorentzian theory.

Barnett (vii. 1128) [29] holds that 'the result follows from Maxwell's theorem based on a much older, though less exact, relativity principle.' As we have already exploded this alleged theorem of Maxwell, we may turn to the advice expressed by Prof. Tate (p. 78), that, in the absence of a Lorentzian proof, 'the safest way to proceed to the result is . . . to assume the field suggested by the theory of relativity,' namely, Ritz's expression $-c^{-1}V\mathbf{v}\mathbf{H}$. Becker goes further and maintains that, while **J** is deducible from the electron theory, **Q** is a 'consequence of the theory of relativity' and 'would never appear if we

[28] A. Einstein and J. Laub, AP 26 (1908) 532; Tolman, iii. 187 and v. 117; Swann, i. 365; Thirring, ii. 339; Becker, p. 334; Tate, p. 79.

[29] He simply assumes the formula for **Q**. He also confuses χ and ψ, i.e. the v′ of s' and the v of the charge. The formula for **Q** is now generally assumed: 'It is well known that when a magnetic dipole with a moment **M** moves with a velocity **v**, an electric moment $c^{-1}V\mathbf{v}\mathbf{M}$ appears.'—J. Weyssenhoff, *Nature* 141 (1938) 329*.

retained the concept of absolute simultaneity.' In fact 'the electric field generated by a moving magnet, known for so long in technology, becomes intelligible only through the relativistic formula, it can be regarded as an immediate consequence of Einstein's definition of simultaneity.' Hence he complains that 'in descriptions of unipolar induction in technical literature it is not made clear that it is a relativistic effect.' [30]

This is merely typical of the exaggerated claims fabricated by followers of Einstein. The reply, which cannot here be developed, may be thus summarised :

(1) The formula follows at once on Ritz's theory, without any special physical assumptions. There is no proof whatever of Becker's assertion that 'the field is of purely electrostatic nature, it comes from the electric polarisation of the magnet.' It is curious too to find a relativist holding that these polarising-electrons exist for a stationary observer but are non-existent for one who is comoving with the magnet.

(2) The magnets used in technology are conductors and for them the effect does not exist, being counteracted by a redistribution of free electricity.

(3) The formula has no connection with unipolar induction. The only experiment bearing on the term **Q** is that of M. and H. A. Wilson, which is not very decisive. In any case the experiment involved a *rotating* magnetised dielectric ; hence it is beyond the scope of the special theory of relativity.[31]

(4) It is not easy to see how a theory which claims to be fundamental can be applied to macroscopic equations (containing $\varkappa$ and μ) which are used only owing to our observational limitations and are based on very complex micro-processes.

(5) The resultant logical situation is puzzling. On the one hand, relativists start by accepting Maxwell's equations—i.e. ultimately Liénard's formula—for experiments in the laboratory ; on the other hand, they claim here that the theory proves a result inconsistent with Liénard's formula.

[30] Becker, pp. 334, 338, 335. Compare his article on 'Unipolar-Induktion als Folge des relativistischen Zeitbegriffs.'—*Naturwiss.* 20 (1932) 917–919.

[31] Silberstein, unlike other relativists, refers to this difficulty : 'In the theoretical treatment of the problem, uniform translation (of each element) can with sufficient accuracy be substituted for the actual spin.'—*Theory of Relativity*, 1924², p. 274. Apart from the difficulty involved in the multiplication of elementary rotating observers, this argument would justify the application of the special theory to motion of every kind.

For these reasons the alleged proof of **Q** from relativity deserves a more critical examination than it has yet received. Though such an investigation is beyond the scope of this volume, we decline to accept the claim made in the following quotations :

> Experiments have been conducted by various physicists, and in particular by H. A. Wilson and A. Eichenwald, with a view to discriminating between the rival hypotheses, [and] in each case the victorious hypothesis is found to be [Einstein's theory of relativity]. —Jeans, p. 605.
>
> Minkowski's electromagnetic equations account fully for the well-known results of Rowland's, Wilson's, Röntgen's and Eichenwald's experiments.—Silberstein, p. 273.
>
> The theory [of relativity] can also be shown to give satisfactory explanations of the Roentgen-Eichenwald experiment on the magnetic field produced by the rotation of a dielectric in an electric field, and of the H. A. Wilson experiment on the surface-charge produced by the rotation of a dielectric in a magnetic field.—Tolman, *Relativity, Thermodynamics and Cosmology*, 1934, p. 116.

14. High-Speed Electrons.

Using the approximate Ritz formula (11.7), we can obtain results accurate only to the order v^2/c^2. Let us apply it to the case of a point-charge (e) moving with velocity v in the x-direction between two infinite planes ($y = \pm h$) parallel to xz, charged to the density $\pm\sigma$ (Fig. 70). We have

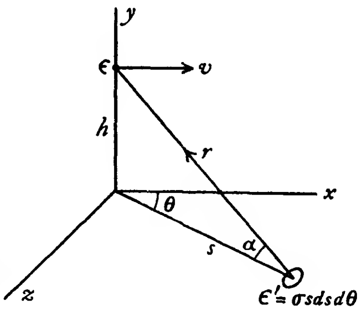

Fig. 70.

$$u = v_x = v \qquad v_y = 0 \qquad u_r = -v\cos\theta\cos\alpha$$

$$\cos\alpha = s/(h^2+s^2)^{\frac{1}{2}} \qquad \sin\alpha = h/(h^2+s^2)^{\frac{1}{2}}$$

$$\delta F_y = \frac{ee'}{r^2}\sin\alpha\left[1 + \frac{3-\lambda}{4}\frac{v^2}{c^2} - \frac{3(1-\lambda)}{4}\frac{v^2}{c^2}\cos^2\theta\cos^2\alpha\right]$$

$$F_y = e\sigma\int_0^{2\pi} d\theta\int_0^{\infty} sds\,\frac{h}{(h^2+s^2)^{\frac{3}{2}}}\left[1 + \frac{3-\lambda}{4}\frac{v^2}{c^2} - \frac{3(1-\lambda)}{4}\frac{v^2}{c^2}\cos^2\theta\,\frac{s^2}{h^2+s^2}\right]$$

$$= 2\pi\sigma e\left(1 + \tfrac{1}{2}\frac{v^2}{c^2}\right).$$

Doubling for the effect of the second plane,

$$F_y = 4\pi\beta'\sigma e = \beta' Ee,$$

where $\beta' = 1 + v^2/2c^2 + \ . \ . \ .$ (12.45)

Applying Liénard's formula (7.17) to the same case, we find, since $v' = 0$,

$$\delta F_y = ee' \cos (ry) \ . \ /r^2.$$

Whence, for the two planes,

$$F_y = 4\pi\sigma e = Ee. \qquad (12.46)$$

Therefore the two theories give different results. Let us see the device of crossed fields, employed by Bestelmeyer, Bucherer, Wolz and others to find F_y experimentally. Beta rays pass between the plates of a condenser, and by means of an outer

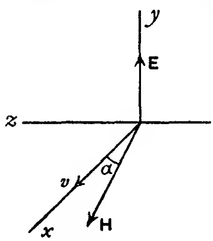

Fig. 71.

solenoid a perpendicular magnetic field is also applied; on emerging, the electrons strike a photographic plate at a measured distance (a) from the condenser. Then (Fig. 71) **E** is the vector $(0, E, 0)$ and **H** is $(H \cos \alpha, 0, H \sin \alpha)$, v being along the x-axis. The acceleration-components along x and z are zero, that along y is zero if

$$E = v/c \ . \ H \sin \alpha. \qquad (12.47)$$

In this case the electron will move in a straight line and so will be able to pass the narrow gap between the condenser-plates. On emerging, it is subject only to H and we ought then to have

$$ev/c \ . \ H \sin \alpha = mf_y.$$

These two equations represent the accepted aether-electron theory, which we have called the Lorentz theory, as expressed in the Liénard-Schwarzschild force-formula for point-charges. And they do *not* agree with the experimental results. Lorentz therefore proposed to modify the right-hand side of the last equation by multiplying it by

$$\beta = (1 - v^2/c^2)^{-\frac{1}{2}} = 1 + v^2/2c^2 + \ . \ . \ .$$

As we have already seen, Lorentz justified this by ceasing to regard the electron as a point-charge, by taking it to be a spherical distribution of sub-electrons which is distorted by its motion through the aether; and Einstein subsequently suggested justifying the alteration by what is really a piece of algebra. But, whatever be the *post factum* argument invented, methodologically the factor β is simply a modification introduced into the

Lorentz theory in order to secure agreement with the experimental results. Accepting it, we have

$$Ee = ev/c \,.\, H \sin\alpha = \beta m f_y. \tag{12.48}$$

The distance traversed is $a/\sin\alpha$, which can be equated to vt as a close approximation. The deviation is

$$y = f_y t^2/2 = ea^2H^2/2mc^2\beta E \tag{12.49}$$

since $f_y = Ee/\beta m$, $\quad t = a/v \sin\alpha$, $\quad v\sin\alpha = cE/H$.

Turn now to Ritz's theory without any *ad hoc* modification. At least as far as second-order terms, H is not changed by the velocity of e; but the electric intensity becomes $\beta' e$. Hence

$$\beta' Ee = ew/c \,.\, H\sin\alpha = mf_y,$$

where we have substituted w for v as the velocity.

Since $f_y = \beta' Ee/m$, $t = a/w \sin\alpha$, $w\sin\alpha = \beta' cE/H$, we have

$$y = f_y t^2/2 = ea^2H^2/2mc^2\beta' E. \tag{12.50}$$

That is, we obtain exactly the same formula as in Lorentz's specially modified theory, with β' substituted for β. It is easily seen that to the second-order, which is the approximation we are at the moment considering, these two quantities are equal. For

$$\begin{aligned}\beta &= 1 + v^2/2c^2 + \ldots = 1 + E^2/2H^2\sin^2\alpha + \ldots \\ &= 1 + w^2/2c^2(1 + w^2/2c^2)^2 \\ &= 1 + w^2/2c^2 + \ldots = \beta'.\end{aligned}$$

Next let us consider the case of e moving with v along y perpendicularly to the plane xz. Applying Ritz's formula (11.7) and putting $u_r = v\sin\alpha$, $u_y = v$, we have

$$\delta F_y = \frac{ee'\sin\alpha}{r^2}\left[1 + \frac{3-\lambda}{4}\frac{v^2}{c^2} - \frac{3(1-\lambda)}{4}\frac{v^2}{c^2}\sin^2\alpha - \frac{1+\lambda}{2}\frac{v^2}{c^2}\right]$$

Whence, as before,

$$\begin{aligned}F_y = 2\pi\sigma e \int s\,ds \Bigg[\frac{h}{(h^2+s^2)^{3/2}}&\left\{1 + \frac{1-3\lambda}{4}\frac{v^2}{c^2}\right\} \\ &- \frac{h^3}{(h^2+s^2)^{5/2}}\cdot\frac{3(1-\lambda)}{4}\frac{v^2}{c^2}\Bigg] \\ = 2\pi\sigma e(1 - \lambda v^2/2c^2).\end{aligned} \tag{12.51}$$

Doubling for the effect of the second plane, we have

$$mv\,dv/dy = mf = F = Ee(1 - \lambda v^2/2c^2)$$

or

$$eE\,dy = mv\,dv(1 + \lambda v^2/2c^2 + \ldots)$$

Hence
$$eV = m\int vdv(1 + \lambda v^2/2c^2 + \ . \ . \ . \ )$$
$$= mv^2/2 \, . \, (1 + \lambda v^2/4c^2 + \ . \ . \ .) \qquad (12.52)$$

Such an experiment is like that of Perry and Chaffee.[32] Let us apply to it the usually accepted modified Lorentz theory :

$$\begin{aligned} eV &= \int_0^v vd(\beta mv) \\ &= mc^2(\beta - 1) \\ &= mv^2/2 \, . \, [1 + 3v^2/4c^2 + \ . \ . \ . \ ]. \end{aligned} \qquad (12.53)$$

Hence if $\lambda = 3$, the two formulae coincide as far as the second-order.

We can express this interesting result in another way. Using formula (11.4) with $\alpha_0 = \gamma_0 = 1$ but allowing the other undetermined coefficients to stand, we easily find instead of (12.45),

$$F = Ee[1 + (\alpha_1 + \alpha_2/3)v^2/c^2].$$

And instead of (12.51) we find

$$F = Ee[1 + (\alpha_1 - \beta_0 + \alpha_2/3)v^2/c^2].$$

We take it that experiment shows that the respective coefficients of v^2/c^2 are $1/2$ and $-3/2$ respectively.

This gives $\beta_0 = 2$; and the other two coefficients are connected by $\alpha_1 + \alpha_2/3 = 1/2$. That is, from these two experiments alone we have gone far to determine the coefficients.

Following Ritz (p. 408), let is now proceed to examine in general the case in which $w = v/c$ is comparable with unity, while $w' \equiv v'/c$ and the acceleration (or rather the quantity rf'/c^2) are small. With our former notation we have

$$\begin{aligned} u^2/c^2 &= \Sigma(w_x - w_x' + rf_x'/c^2 + \ . \ . \ .)^2 \\ &= w^2 - 2\Sigma w_x w_x + w'^2 + 2r\,(\Sigma w_x f_x')/c^2 + \ . \ . \ . \\ &= \text{say, } w^2 + \varepsilon. \\ u_\rho/c &= w_r - w_r' + (. \ . \ .)/c^2 + \ . \ . \ . \\ &= \text{say, } w_r + \eta \\ \rho \ &= r(1 + rf_r'/2c^2). \end{aligned}$$

[32] PR 36 (1930) 904. The velocity v of cathode rays, driven by potentials of 10^4 to 2×10^4 volts, is measured directly by timing the passage of the electrons between two localised transverse high-frequency electric fields 75 cm. apart. Electrons which pass undeflected travel the distance in an even multiple of half a cycle of the oscillating fields. Since the plate-charges are not stationary, our use of Ritz's formula in the text does not apply to this experiment. See also Kirchner, AP 8 (1931) 975.

We shall now develop the functions A and B of formula (11.3) by Taylor's series in the neighbourhood of the values $u_\rho/c = w_r$ and $u^2/c^2 = w^2$.

$$\begin{aligned} A(w_r + \eta, w^2 + \varepsilon) &= A(w_r, w^2) + \eta \frac{\partial A}{\partial w_r} + \varepsilon \frac{\partial A}{\partial w^2} \\ &\quad + \tfrac{1}{2}\eta^2 \frac{\partial^2 A}{\partial w_r^2} + \eta\varepsilon \frac{\partial^2 A}{\partial w_r \partial w^2} + \tfrac{1}{2}\varepsilon^2 \frac{\partial^2 A}{(\partial w^2)^2} + \ldots \\ &= A - 2\Sigma w_x w_x' \,.\, \partial A/\partial w^2 - w_r' \partial A/\partial w_r + c^{-2}(\ldots), \end{aligned}$$

where A stands for $A(w_r, w^2)$. Similarly the other function can be developed. Hence, neglecting the relatively small terms with the factor c^{-2}, formula (11.3) becomes

$$\begin{aligned} F_x/(ee'/r^2) = \cos(rx)[A &- (\Sigma w_x w_x')\partial A/\partial w^2 - w_r' \partial A/\partial w_r] \\ &- (w_x w_r - w_r w_x' - w_x w_r')B \\ &+ 2w_x w_r (\Sigma w_x w_x') \partial B/\partial w^2 \\ &+ w_x w_r w_r' \partial B/\partial w_r. \end{aligned} \qquad (12.54)$$

Hence for electrostatic action ($w' = 0$)

$$F_x = ee'/r^2 \,.\, [A \cos(rx) - w_x w_r B]. \qquad (12.55)$$

Hence, when $w = v/c$ is comparable with unity, this is not equal to $ee'/r^2 \,.\, \cos(rx)$.

Next let us examine the action of an element ds' of a closed neutral current; the conductor or magnet (and hence the positive ions) being at rest and $J_x' = e'v_x'/c = e'w_x'$. Summing up the action of the two kinds of ions on e as given by (12.54), the terms independent of w' cancel and we obtain

$$\begin{aligned} dF_x/(J'ds'e/r^2) = & \\ &- \cos(rx)[2\{\Sigma w_x \cos(xds')\}\partial A/\partial w^2 + \cos(rds')\partial A/\partial w_r] \\ &+ B[w_r \cos(xds') + w_x \cos(rds')] \\ &+ 2w_x w_r \{\Sigma w_x \cos(xds')\} \partial B/\partial w^2 \\ &+ w_x w_r \cos(rds')\, \partial B/\partial w_r. \end{aligned} \qquad (12.56)$$

Hence the action of the current is proportional to J' and two elements of equal and opposite currents have no action; so that, as in electrodynamics, a closed current is equivalent to a magnetic shell. Also, the force being a linear function of the direction-cosines of ds', the principle of sinuous currents is satisfied. But for w comparable with unity the action of a closed current on an

electron is not in general perpendicular to the velocity $v = wc$ of the electron. The component of F parallel to w is

$$\int \Sigma dF_x w_x/w = -J'e/w . \int \Sigma X dx',$$

the quantities X, Y, Z being easily found. This is zero for every closed circuit only if curl (X, Y, Z) is zero. This means that A and B must satisfy a third-order differential equation, which is automatically true when w is very small. Hence the knowledge of the 'magnetic field'—or the force on a slow-moving charge—at a point is not sufficient to determine the force which a rapidly moving electron would experience at the point—unless a certain relation is satisfied. Thus experiment alone can decide whether the ordinary laws of electromagnetics are applicable to beta-rays.

Consider more generally the case, already examined, of an electron (e) moving along x between two charged plates ($\pm\sigma$) parallel to xz. We have very approximately

$$w_y = w_z = 0, \quad w_x = w, \quad w_r = w \cos(rx).$$

Hence from (12.54), putting $dx'dz' = dS$,

$$F_x/e\sigma = \int dS r^{-2}[A \cos(rx) - Bww_r],$$

$$F_y/e\sigma = \int dS r^{-2} A \cos(ry),$$

$$F_z/e\sigma = \int dS r^{-2} A \cos(rz). \tag{12.57}$$

To a first approximation the extent of the condenser may be considered very large; the integral may be extended to the whole plane $x'z'$ first for $y' = 0$ and then for $y' = a$, and the results subtracted. Since we have assumed that A and B are even functions of w_r, the integrand in the first and third integral has opposite values for the points $(x - x', z - z')$ and $(x' - x, z' - z)$. Therefore these integrals are zero and the force is parallel to y. Using polar coordinates $(x - x' = r\cos\theta, w_r = w\cos\theta)$, we have

$$F_y = e\sigma(y - y')\iint A \sin\theta d\theta dr . r^{-1}[r^2 \sin^2\theta - (y - y')^2]^{-\frac{1}{2}}.$$

The integral is 4 times that of the portion between the lines $x' = 0$ and $z' = 0$, *i.e.* $r = (y - y')/\sin\theta$, $r = \infty$ and $\theta = 0$, $\theta = \pi/2$. Now

$$(y - y')\int dr r^{-1}[r^2 \sin^2\theta - (y - y')^2]^{-\frac{1}{2}}$$
$$= \text{arc tan}\,[\{r^2 \sin^2\theta - (y - y')^2\}^{\frac{1}{2}}/(y - y')].$$

At the lower limit this is zero. At the upper limit ($r = \infty$): it gives $\pi/2$ for $y - y' > 0$ (the first plate being $y' = 0$); and for $y - y' < 0$ (the second plate being $y' = a$), it gives $-\pi/2$. Therefore

$$F_y = 4\pi e\sigma \int_0^{\pi/2} A(w^2, w \cos \theta) \sin \theta d\theta,$$

or

$$F = Ee \int_0^1 A(w^2, wp)dp. \tag{12.58}$$

Let us next with the help of (12.56) find the magnetic action:

$$R_y = -eJ' \int r^{-2}[2w \cos (ry) \,.\, \partial A/\partial w^2 \,.\, dx' - wB \cos (rx)dy'$$
$$+ \cos (ry) \,.\, \partial A/\partial w_r \,.\, \Sigma dx' \cos (rx)]$$

$$R_z = -eJ' \int r^{-2}[2w \cos (rz) \,.\, \partial A/\partial w^2 \,.\, dx' - wB \cos (rx)dz'$$
$$+ \cos (rz) \,.\, \partial A/\partial w_r \,.\, \Sigma dx' \cos (rx)]. \tag{12.59}$$

The integrals are extended to all the currents, including those equivalent to the magnets. The functions A and B depend only on $\cos \theta = (x - x')/r$ and are even functions of this argument. The electron moves sensibly along the x-axis, and there is symmetry with respect to the y-axis. Hence, if we change y' into $-y'$ and dy' into $-dy'$, the actions of the corresponding elements cancel in R_y, which accordingly is zero. Also the change produced in the velocity being small relatively to the initial velocity, a force parallel to this velocity (i.e. along x) is negligible to a first approximation. Therefore the magnetic action observed is perpendicular to the field and to the velocity, as in the Lorentz theory. In the expression for R_z in (12.59) put

$$A = 1 + (3 - \lambda)w^2/4 - 3(1 - \lambda)w_r^2/4, \quad B = (3 - \lambda)/4,$$

and we obtain

$$R_y = -eJ'v/2c \,.\, \int ds' r^{-2}\Big[(3 - \lambda) \cos (xds') \cos (rz)$$
$$- (1 + \lambda) \cos (zds') \cos (rx) - 3(1 - \lambda) \cos (rz) \cos (rx) \cos(rds')\Big].$$

The portion of the integral multiplied by $-\lambda$ is

$$\int ds' r^{-2}[\cos (xds') \cos (rz) + \cos (zds') \cos (rx)$$
$$- 3 \cos (rz) \cos (rx) \cos (rds')].$$

This is easily seen to be zero by the same argument as transforms (4.6) into (4.7), on substituting the direction z for the direction ds. So the expression reduces to

$$R_y = -eJ'v/c \,.\int ds'r^{-2}[\cos(rz)\cos(xds') - \cos(rx)\cos(zds')]$$

$$= eJ'v/c\int r^{-2}(V\mathbf{ds}'\mathbf{r}_1)_y$$

$$= v/c \,.\, H_y \text{ from } (4.1)$$

$$= c^{-1}(V\mathbf{vH})_y.$$

But if we do not confine A and B to the initial terms of an expansion in series, the force on the electron, while still in the y-direction, will have a value different from that ordinarily assumed. It is obvious however that, without in any way interfering with the explanation of ordinary electromagnetic phenomena already given, Ritz's theory can without any difficulty account for the Kaufmann-Bucherer results.

Similarly, for the second case already considered, e moving along y perpendicularly to the condenser plates, we have

$$w_x = w_z = 0, \quad w_y = w = v/c, \quad w_r = w\cos(ry).$$

Whence $F_x = F_z = 0$ and

$$F_y = e\sigma\int dSr^{-2}\cos(ry)(A - Bw^2)$$

$$= e\sigma(y-y')\,.\iint\frac{d\theta dr(A - Bw^2)\sin\theta.}{r[r^2\sin^2\theta - (y-y')^2]^{\frac{1}{2}}}$$

$$= 4\pi e\sigma\int_0^{\pi/2}[A - Bw^2]\sin\theta d\theta, \qquad (12.60).$$

where A and B are functions of w^2 and $(w\sin\theta)^2$. Or otherwise

$$F = eE\int_0^1 (A - Bp)(1-p^2)^{-\frac{1}{2}}pdp = eEf(w^2),$$

where A and B are functions of w^2 and w^2p^2. Putting

$$F = mvdv/dy = mc^2wdw/dy,$$

we have

$$eV = \int eEdy$$

$$= mc^2\int_0^w wdw/f(w^2).$$

Assuming that the experimental results demand it, we may take this integral to be $(1-w^2)^{-\frac{1}{2}} - 1$, so that $f(w^2) = (1-w^2)^{3/2}$. We

have already shown that, so far as ordinary electromagnetic experiments are concerned, this merely involves taking $\lambda = 3$.

Ritz (p. 442), concludes accordingly:

> This theory of the variability of electrodynamic mass is based on the weakest points of Lorentz's theory. We can explain the observations just as well and even better by suitably changing the velocity-terms in the expression for the forces so as to introduce only relative motions.

This was written in 1908. And in 1927 Prof. Bridgman wrote as follows (i. 137, 139, 141):

> We may . . . inquire whether the equations are correct in stating that the force acting on a charge moving in an electric field is simply the product of the charge and the field strength. . . . As a matter of fact, in order to determine electrical mass, we have to use that equation which we are now engaged in trying to establish. Logically we have again the vicious circle, the physical significance of which is that independent operations do not exist for giving unique meaning to the concept of force on a charge at high velocity. . . .
>
> It should be possible by arbitrary definition to make this force any function of velocity that we please—of course reducing to the proper value at low velocities—and then to determine the other equations so that the entire group of equations is consistent with experiment. So far as I know, no one has tried to give such a modified set of equations; and indeed there is no particular reason why anyone should bother to do this, since the present equations are simple enough.

Nineteen years before this was published Ritz had given his formulae to the world; apparently they have not yet been heard of by distinguished physicists.[33] Ritz did more than fulfil the desideratum here expressed, for he confined his formulae to the purely relative velocity of pairs of electrons. Indeed, the modification suggested by Bridgman is quite impossible, for any second-order change in the Lorentz-Liénard formula, which involves aether-velocities, would be incompatible with Ampère's results.

Ritz's achievement is indeed remarkable. For in the first place, it is in full conformity with what used to be called 'relativity' before Einstein appropriated the word to denote a new and highly sophisticated notion. Apart from the details of Ritz's formula, there emerges a very interesting general fact.

[33] It is understood of course that in the present volume we are not specifically dealing with any *optical* objections to Ritz's ballistic theory.

The phenomena, which on the hypothesis of a force-law involving absolute velocities can be explained only by the assumption of a variable mass, are equally explicable by a force-law dependent only on relative velocity *without* assuming variable mass. In the second place, Ritz's result does not involve any hypothesis concerning the internal constitution of an electron, i.e. a continuous or sub-electronic distribution of charge kept together by specially postulated forces; the electron can still be treated as a point-charge. Of course, one may answer with Prof. Bridgman that 'there is no particular reason why anyone should bother' with all this since the accepted theory is 'simple enough.' It is impossible to argue against such self-satisfaction.

The only explanation possible on the ordinary theory is to assume:

(1) the electron is not a point-charge but a rather miraculously-subsisting spherical aggregate of charges;
(2) the aether is comoving with the earth so that the v of the electron is the velocity relative to the laboratory;
(3) the mass of the electron is electromagnetic in origin;
(4) owing to the motion through the aether the dimensions of the electron become flattened in the ratio $(1 - w^2)^{\frac{1}{2}}$ in the direction of motion.

This theory has already been expounded and criticised in Chapter VIII.

CHAPTER XIII

The Aether

1. The Mechanical Aether.

The undulatory theory of light, thanks largely to the work of Fresnel, gradually supplanted the emission theory as formulated by Newton. Even as early as 1760 Euler could write confidently :

> Light is nothing but an agitation or disturbance caused in the particles of the aether. . . . The parallel between light and sound is in this respect so well established that we can boldly maintain that if the air became as subtle and at the same time as elastic as the aether, the velocity of sound would also become as rapid as that of light.—*Lettres à une Princesse d'Allemagne*, no. 20, Paris, 1843, p. 72.

The self-confident dogmatism displayed in this quotation was as typical of the mechanical as of the later electromagnetic theory. Here is a characteristic story of Cauchy :

> Cauchy was walking with Père Gratry in the Luxembourg gardens. They were discussing the next life, the happiness of the elect in finally knowing, without restriction or hindrance, truths pursued in this world with such slowness and difficulty. Alluding to Cauchy's researches on the mechanical theory of the reflection of light, Gratry suggested that one of the greatest joys of the illustrious geometer in heaven would doubtless be to penetrate the secret of light and to understand intimately these problems of optics which had been the objects of his meditation. But Cauchy objected ; on this point he considered he could not admit that one could ever learn anything more than what he knew ; he could not conceive that the most perfect intelligence could comprehend the phenomenon of reflection otherwise than he had expounded it. He had given a mechanical theory of it ; his piety did not go so far as to believe that it would be possible for God Himself to do anything else or to do it better.—B. Brunhes, *La dégradation de l'énergie*, 1922, p. 261.

This is cited not merely for its historical interest but chiefly to illustrate a type of mind which, minus the piety, is by no means

defunct in contemporary physics. Equally apposite in these days of 'relativity' is the slashing and successful criticism of Cauchy which MacCullagh (pp. 207, 209) published in 1841 :

> That a theory involving so many inconsistencies should have been advanced by a person of M. Cauchy's reputation, would perhaps appear very extraordinary, if we did not recollect that it was unavoidably suggested by the general principles which he had previously adopted, and which were supposed not merely by himself but by the scientific world generally to have already afforded the only satisfactory explanation of the laws of double refraction. . . . The hypothesis was embraced by a great number of writers in every part of Europe, who reproduced each in his own way the results of M. Cauchy, though sometimes with considerable modifications. Every day saw some new investigation purely analytical, some new mathematical research uncontrolled by a single physical conception, put forward as a 'mechanical theory' of double refraction, of circular polarisation, of dispersion, of absorption; until at length the Journals of Science and the Transactions of Societies were filled with a great mass of unmeaning formulas.

Some of MacCullagh's own formulae, which have survived all the subsequent vicissitudes of optical theory, illustrate the important point that the essence of a physical theory lies in its equations.[1] Thus the discovery of conical refraction [2] confirmed the correctness of Fresnel's equation for the wave-surface in crystals; but it did not confirm the elastic-solid theory of light. Maxwell himself (iv. 767) regarded 'the aether as possessing elasticity similar to that of a solid body and also as having a finite density.' He assumed (ii. 274), 'kinetic energy to exist wherever there is magnetic force,' and 'this energy exists in the form of some kind of motion of the matter in every portion of space.' In 1855 he declared his ambition as follows (iii. 188): 'By a careful study of the laws of elastic solids and of the motions of viscous fluids, I hope to discover a method of forming a mechanical conception of this electrotonic state adapted to

[1] 'I chose to publish the equations without comment as bare geometrical assumptions. . . . A mechanical account of the phenomena still remained. A desideratum which no attempts of mine had been able to supply.'—MacCullagh (1841), p. 198.

[2] 'Whatever may be the strength which the theory of gravitation derives from the discovery of Neptune, it is matched by the strength which the undulatory theory derives from the discovery of conical refraction.'—Tyndall, *Notes on Light*, 1873[5], p. 74. *Cf.* H. Lloyd, *Lectures on the Wave Theory of Light*, 1841 p. 54.

general reasoning.' But he failed; electrotonic intensity remained and remains merely the magnetic vector potential. All Maxwell's analogies with elastic solids are entirely irrelevant to his theory.

Our text-books of course continue to represent demoded ideas as 'modern' views:

> According to modern views, actions in an electric field require a medium for their transmission, and charges at a distance can only affect one another by means of stresses in this intervening medium. It is usual to speak of this medium as the ether.—Ramsey, p. 19.

And the old elastic and hydrodynamical analogies are unfortunately not yet extinct. We find even H. A. Lorentz talking as follows:

> We may now try to interpret one group of Maxwell's equations by taking the components of the magnetic force to be proportional to the displacements in the aether. . . . We can avoid this difficulty by identifying with the aether rotations not the displacement current but the dielectric displacement itself. . . . Thus, wherever there is a magnetic force, we must imagine an aether velocity in the direction of this force and proportional to it, and we have to look for the dielectric displacement in the rotation due to or associated with that velocity. . . . The aether has thus to be attributed the property that its potential energy is proportional to the square of the rotation of its particles.—Lorentz, xi. 26–28.

Poynting (p. 674) could seriously ask in 1903: 'What then are the electric strain and the magnetic spin which now we suppose to constitute light?' And we are hopefully told in the latest edition of a favourite text-book:

> The theory which promises most favourably at present is that which regards the ether as a turbulent fluid and light as an electromagnetic phenonemon arising from very rapid alternating electric polarisations or 'displacements' as Maxwell termed them.—T. Preston, *Theory of Light*, ed. A. Porter, 1928[5], p. 34.

But it has now become generally admitted that the obscurities and inconsistencies in Maxwell's work are due to the conjunction of two very different tendencies. The first is the attempt to explain electrical actions by the properties of the hypothetical medium which is their carrier; this, with the accessory hypotheses involved, must be pronounced a failure. The second is a purely phenomenological description by means of partial differential equations based on the assumption of certain vectors specifying

the electric and magnetic state of a body. The latter alone survives to-day.

But this disassociation involved considerable emotional shock. Lord Kelvin never succeeded in adjusting himself (iii. 9):

> A real matter between us and the remotest stars I believe there is, and that light consists of real motions of that matter, motions just such as are described by Fresnel and Young, motions in the way of transverse vibrations. If I knew what the electromagnetic theory of light is, I might be able to think of it in relation to the fundamental principles of the wave-theory of light. But it seems to me that it is rather a backward step from an absolutely definite mechanical notion that is put before us by Fresnel and his followers, to take up the so-called electromagnetic theory of light in the way it has been taken up by several writers of late.

But even he in the end confessed his failure. In his Jubilee confession in 1896 he declared [3]:

> One word characterises the most strenuous of the efforts for the advancement of science that I have made perseveringly during fifty-five years: that word is failure. I know no more of electric and magnetic force, or of the relation between ether, electricity and ponderable matter, than I knew and tried to teach to my students of natural philosophy fifty years ago in my first session as Professor.

In spite of the quasi-mystical views of a few people like Sir Oliver Lodge, this confession may be taken to be the epitaph of all attempts to picture, visualise, describe, analogise or classify the aether. Physics has come to adopt the view which Duhem taught so perseveringly:

> A physical theory is not an explanation; it is a system of mathematical propositions deduced from a small number of principles whose object is to represent a group of experimental laws as simply, completely and exactly as possible. The sole test of a physical theory, which allows us to pronounce it good or bad, is the comparison between the consequences of the theory and the experimental laws.[4]

[3] Thompson, *Life of William Thompson*, 1910, p. 1072. *Cf.* Michelson, *Light Waves and their Uses*, 1903, p. 161: 'I may quote a statement which Lord Kelvin made in reply to a rather sceptical question as to the existence of a medium about which so very little is supposed to be known. The reply was: "Yes, ether is the only form of matter about which we know anything at all."'

[4] Duhem in Manville, p. 23. *Cf.* Dirac, *Principles of Quantum Mechanics*, p. 7: 'The only object of theoretical physics is to calculate results that can be compared with experiment; and it is quite unnecessary that any description of the whole course of the phenomena should be given.'

The adoption of this standpoint is not due to any positivist bias; it has no connection with any particular brand of philosophy; it merely defines the function of physics, a science which is concerned with measure-numbers. It is the outcome of hard experience, for we have found that the world is not built on a simple elastic-solid model and that 'acceptable and demonstrable facts do not in the twentieth century seem to be disposed to wait on suitable mechanical pictures.'[5] Once we grasp the proposition that the essential content of physics is the relations of measure-numbers leading to experimental control, we can relegate to philosophers and popularisers a vast amount of discussion which has no scientific significance. In particular we can 'debunk' the idea of an aether, expelling its irrelevant emotional and pictorial content. So far as physics is concerned, any debate concerning its existence or function must confine itself to a discussion of quantitative equations, for it is only through such that the aether can become an ingredient of the science of physics. Judged by this standard, practically all the arguments for and against an aether have nothing whatever to do with physics.

Here are some pronouncements against the aether:

> It is probable that the future historian of physics will be astounded that the vast majority of physicists should accept a system of such bewildering complexity and precarious validity rather than abandon ideas which seem to have their sole origin in the use of the word 'aether,' and reject those to which so many lines of thought point insistently.—N. Campbell, PM 19 (1910) 189.
>
> A certain obsession had taken a strong hold upon the minds of the physicists; I refer to the luminiferous ether. . . . Now that we are suddenly freed from this obsession, we feel as if awakened from a hideous nightmare.—G. N. Lewis, *The Anatomy of Science*, 1926, p. 75.
>
> It is now time to see that the aether has played out its historic part and that it has the right to a place of honour only in the history of physics.—Frenkel, i., p. vii.

Are these writers discussing quantitative physics? Does their seemingly bold advocacy lead them to propose a single change in the accepted equations of electromagnetics? Not at all. We must not take their vehement protestations quite so literally.

[5] R. A. Millikan, *Nature*, 127 (1931) 170. His next sentence is: 'Indeed has not modern physics thrown the purely mechanistic view of the universe root and branch out of its house?' Has it?

They all accept the aether in the only sense in which the word has any effective meaning in physical science. They appear merely to be irritated with the word 'aether' and with the irrelevant observations of elastic-solid diehards. On the other hand, when protagonists tell us that 'the abandonment of the aether leads to epistemological difficulties,[6] or that 'a physics without the aether is no physics,'[7] or that 'the ether is not a fantastic creation of the speculative philosopher, it is as essential to us as the air we breathe,'[8] they are expressing their personal faith in something, they are professing a conviction which is outside the scope of physics as a science. Sir Arthur Eddington[9] gaily declares that 'among leading scientists to-day about half assert that the aether exists and the other half deny its existence; but as a matter of fact both parties mean exactly the same thing and are divided only by words.' The position is certainly one neither for self-congratulation nor for witticism. It is high time to end this word-battle, to remove the conflict from the arena of popular philosophy, and to test it by the formulae of scientific physics.

2. The Electromagnetic Framework.

The current fashion among leading physicists is quite unjustifiable. They either apologise[10] for the aether as just a convenient 'background' or they profess to deny it with considerable heat and rhetoric. But surely the issue provides an opportunity for examining our equations, not our consciences. The assertion of an aether in physics must express itself in some characteristic of these equations; its denial must simply mean the production of alternative equations from which this characteristic is absent. A dispute about anything else has nothing to do with physics.

[6] W. Wien, *Neuere Entwicklung der Physik*, 1919, p. 53.

[7] J. Stark, *Die gegenwärtige Krisis in der deutschen Physik*, 1922, p. 11.

[8] Sir J. J. Thomson, *B.A. Report* (*Winnipeg*), 1909, p. 15.

[9] Eddington, *Science and the Unseen World*, 1929, p. 42.

[10] 'In respect to waves of light, the material ether has retreated to an indeterminate position in the background and is rarely talked about.'—Whitehead, *Science and the Modern World*, 1926, p. 184. 'The ether, intangible as its name suggests, has come to be regarded as no more than a convenient fiction introduced to ease the minds of physicists.'—*Outline of Atomic Physics*, by Pittsburgh Univ. Physics Staff, 1933, p. 254.

Let Eddington represent the apologists :

> The aether has ceased to take any very active part in physical theory and has, as it were, gone into reserve. A modern writer on electromagnetic theory will generally start with the postulate of an aether pervading all space ; he will then explain that at any point in it there is an electromagnetic vector whose intensity can be measured ; henceforth his sole dealings are with this vector, and probably nothing more will be heard of the aether itself. In a vague way it is supposed that this vector represents some condition of the aether and we need not dispute that without some such background the vector would scarcely be intelligible—but the aether is now only a background and not an active participant in the theory.—*Space Time and Gravitation*, 1920, p. 29.

Now the fundamental formula logically, even if only implicitly, accepted by ' a modern writer on electromagnetic theory ' is the Liénard-Schwarzschild formula for the force between two charged particles, which involves their absolute velocities. That is, the measurable force between them is taken to involve something more than their relative velocity, it depends on their velocities with respect to some medium, framework or ' background.' Call it what you will, the kinematic relations between it and our two charges actively and effectively intervene in our measure. You may indeed say[11] that ' during the last century all the properties which would make the aether akin to any known fluid have had to be abandoned, one by one.' But, so long as you accept the Maxwell-Lorentz theory, you cannot affirm that ' the aether is not in itself a subject for physical measurement.' Why even Coulomb's law in electrostatics does not hold, on this view, unless both charges are at rest in the aether. No one, except a few belated elastic-solid enthusiasts, holds that we can talk about the density or elasticity of the aether ; for these alleged properties do not enter into electromagnetics. But in current expositions, even if after some general phrases ' nothing more will be heard of the aether itself,' it is *there* all the time ; whether explicitly recognised or not, velocities with respect to the aether occur on almost every page. The fundamental formula on which the whole exposition depends definitely involves these velocities ; they are an integral part of physics as thus expounded. And this is not, as some writers tell us, a mere matter of mathematical convenience.

[11] Eddington, *Mind*, 29 (1920) 146.

> All that we really recognise in this and in other theories are the different related performances of pieces of matter; and we can only describe them in relation to a material framework (real or imaginary) plotted out with material rulers. What happens in the space between (space is that something which can be occupied by matter) we do not and cannot know. But in order to simplify our description of the observed relations it is convenient and helpful to imagine the aether existing throughout our framework and behaving in the requisite manner.—Livens, ii. 388 f.

It is something to realise that the fundamental phenomenon is the force between charges. But it is surely taxing our credulity to regard Lorentz's theory as merely a convenient and helpful expedient imagined for the purpose of simplifying our description, except in the sense that every physical theory may be so regarded. At least this particular theory makes the force between two charges—and consequently all electromagnetic formulae—depend on their kinematic relation to something else. And this something else, the aether, cannot be got rid of except by adopting an entirely different theory such as Ritz's. Bucherer [12] long ago expressed the view that 'Lorentz's equations should always be applied on the supposition that the coordinates are at rest relative to the point whose motion is being studied,' i.e. that Lorentz's theory can be held without an aether. This is easily seen to be incorrect by putting $v' = u$, $f' = 0$, $v = 0$ in the force-formula (7.17). We then have

$$F_x = \frac{ee' \cos (rx)}{r^2}\left(1 + \frac{u^2 - 3u_r^2}{2c^2}\right),$$

whereas the reaction on the other charge e' is $F'_x = - ee' r^{-2} \cos (rx)$. Now in Ritz's formula (11.7) put $f' = 0$ and $\lambda = -1$. We obtain

$$F_x = \frac{ee' \cos (rx)}{r^2}\left(1 + \frac{2u^2 - 3u_r^2}{2c^2}\right).$$

And, since this latter has been shown to give the correct expression for the action between two current-elements, it is clear that Bucherer's expression does not; it is incompatible with experimental facts. Hence if, with Einstein, Eddington and the rest, we accept Lorentz's equations, we can obtain the correct result for the force between two circuits only by adopting for our elementary force-formula an expression which involves

[12] PZ 7 (1906) 553. *Cf.* Ritz, p. 366.

velocities relative to the aether. Try to expel it with a fork—*tamen usque recurret*!

Yet contemporary writers while willing to wound the aether are afraid to strike it. Prof. Jeffreys expresses the widespread attitude :

> The denial of action at a distance in this sense does not carry with it the acceptance of the notion of an ether. The latter concept was effectively that of an elastic solid capable of transmitting transverse waves with a constant velocity, and has broken down under later work. But the ideas of position coordinates and time, and of the electric and magnetic forces associated with them, arise of themselves, quite independently of the assumption of a quasi-material substance filling space. Our knowledge of electromagnetic phenomena indicates that they are related by differential equations, which in turn imply and explain the properties of light. The question of an ether does not arise.—*Scientific Inference*, 1931, p. 211.

Well, of course, if you *define* the aether to mean an elastic solid transmitting vibrations like an iron rod, there is an end of it ; that admittedly has nothing to do with Lorentzian electrodynamics. But the *something* remains ; something which enters physically into the equations, without which the v and v' in our force-formula cannot be defined or measured. You can hardly say that these velocities 'arise of themselves,' like Venus from the foam of the sea ; especially as in Ritz's alternative theory they do not 'arise' at all. Once more let us remember that we are discussing physics as expressed in equations. If the words we use are popularly charged with irrelevant pictorial associations, we must be careful not to foist these notions into quantitative science. If when discussing electromagnetics you reject the aether in the sense of an elastic solid, you are not doing anything wonderful ; you are merely purifying your vocabulary. But if, when expounding Lorentz's theory, you deny the aether in the only relevant sense—namely, the reference-system for the absolute velocities which occur in the formulae—you are unconsciously talking nonsense ; or else you are suffering from suppressed Ritzism.

There is really nothing new in this view, for it has long since become a commonplace that all that we know of the aether is summed up in Maxwell's equations—his quantitative final results, not his arguments and metaphors.

To the question, What is Maxwell's Theory ? I know of no shorter or more definite answer than the following : Maxwell's

theory is Maxwell's system of equations. . . . Maxwell arrived at them by starting with the idea of action-at-a-distance and attributing to the ether the properties of a highly polarisable dielectric medium. We can also arrive at them in other ways. But in no way can a direct proof of these equations be deduced from experience. It appears most logical therefore to regard them independently of the way in which they have been arrived at, to consider them as hypothetical assumptions, and to let their probability depend upon the very large number of natural laws which they embrace.—Hertz, i. 21, 138.

The true function of the ether is merely to assist the mind to a clearer understanding of the sequences of these phenomena. Nothing more is to be predicated of it than the laws that express concisely how these sequences are unfolded. The ether of the electromagnetic theory is to the scientist now nothing more than a vague substratum whose only properties are specified by a number of mathematical equations which will always be associated with the name of Clerk Maxwell.—E. Cunningham, in Pearson's *Grammar of Science*, 1911[3], p. 357.

What in fact we do know about the ether is summed up in Maxwell's equations or in recent adaptations of his equations such as those due to Lorentz.—Whitehead, *Principles of Natural Knowledge*, 1919, p. 22.

It cannot be doubted that all that we know about the aether is contained in Maxwell's electromagnetic theory, and everything else pertains to pure speculation.—W. Wien, AP 65 (1898) p. i.

Clerk Maxwell summed up our whole knowledge of the aether as far as it went in his time.—Lenard, v. 339.

Now we have argued at considerable length that, especially in the light of our knowledge of electrons, Maxwell's differential equations can no longer be regarded as fundamental. The basic quantitative statement of the theory, from which all the phenomena can be synthetically reconstructed, is Liénard's force-formula. Hence the modern version of Hertz's standpoint is the assertion that our scientific knowledge of the aether is contained in that formula. Inasmuch as practically all those who nowadays deny the aether are enthusiastic—one might almost say rabid—believers in the electron-theory which is summed up in Liénard's formula, their denial lacks all scientific content. It must be merely the exhibition of an intense dislike of the word 'aether,' presumably owing to its previous and popular associations. Let us then, merely for the purpose of this chapter and without any desire to cumber the vocabulary of physics, invent a new colourless term. Let us use the Greek word *schesis*—which means *inter alia* a relation—to denote the frame

to which the velocities v and v' of the Liénard formula are referred. These velocities then, instead of being called 'absolute'—another term which ruffles the feelings of many physicists—will be termed *schesic* velocities. Accordingly this formula has the peculiarity of involving schesic velocities; and it is entirely a matter for experiment to determine the appropriate schesis. Ritz's formula, on the other hand, involves only the *relative* velocity of the two point-charges.

We have now narrowed the purely scientific issue to the question of the fundamental force-formula. There are two alternatives: (1) A physicist adopts Liénard's formula and *therefore* upholds the schesis. His verbal denials of something called the 'aether' are entirely irrelevant; when he makes them, he is not speaking as a physicist; he must be writing as a historian or as a philosopher. (2) A physicist adopts Ritz's formula; therefore he uses only a purely relative velocity and does not employ the schesis in his physics.

If, in the light of this simple dilemma, one re-reads the Maxwell-Lorentzian anti-aetherists, one can easily see that their denials are devoid of scientific meaning even for themselves. What do they think they are repudiating? If it is an infinite jelly or a congeries of vortices, they are wasting their time; there is no use in slaying the slain. Most certainly they are not denying the schesis, the frame to which, on Lorentz's theory, velocity, both of moving charges and of light, is referred. Most probably what they are reacting against is their own past. Bertrand Russell professes to be an extreme relativist to-day. But in his younger days he declared sarcastically that

> among all those who have upheld the relativist theory, there has been no one who has developed it in detail—it is principally to this fact that we must attribute its popularity.—*Bibl. Congrès Int. Philos.* 3 (1901) 250.

His position to-day is this:

> Throughout all the revolutions which physics has undergone in the last fifty years, these equations [of Maxwell] have remained standing. Indeed they have continually grown in importance as well as in certainty, for Maxwell's arguments in their favour were so shaky that the correctness of his results must almost be ascribed to intuition.—Russell, *The ABC of Relativity*, 1925, p. 74.

Here at any rate we have one doughty champion of Liénard's schesic velocities against Ritz's relative velocity.

M. Langevin, an anti-aether relativist to-day, seems once to have sufferred from excessive Maxwellianism. For in 1904 he wrote about 'the electromagnetic aether, different from, simpler than and anterior to matter,' which 'seems completely known after the work of Maxwell and of Hertz.'[13] And next year he discussed (iv. 679) 'the production of a magnetic field by an electrified particle in motion with respect to the aether.'

There was a time when Sommerfeld[14] held that 'the aether in a conductor behaves as a viscous fluid, in a non-conductor as a rigid body'; and 'to Maxwell's displacement there corresponds, in our representation, the rotation by which an aether-particle is moved from its position of rest.' He certainly knew a lot about the aether then; to-day he advocates 'etherless optics.'

> The principle of the constancy of the velocity of light has been amply confirmed by observation. This principle states that when once the light has left its source, it propagates itself without any recollection of its origin, in accordance with the laws of the optical field, that is, in all directions with the same velocity *c*. . . . This state of affairs is expressed most directly by the idea of the ether; the source of light, once it has excited the ether, has no influence on the further process. Even if, after the observations of Michelson and Morley and others quoted above, we may no longer recognise the ether, yet we must take over the advantageous features of ether into the realm of etherless optics. We do this by setting up the principle of the constancy of the velocity of light in the above sense, which is thus to be regarded as a condensate and an indispensable remainder of ether physics.—Sommerfeld, *Atomic Structure and Spectral Lines*, 1923, p. 455 f.

We are not here concerned with optics. But we may point out that the constancy of light-velocity means, with reference to the Maxwell-Lorentz equations employed by Sommerfeld, constancy of velocity of light-waves and of potential-waves relatively to the schesis. He has taken over not the remainder but the whole of 'schesic' physics.

In 1905 Sir James Jeans asked us to 'examine the statistical mechanics of a universe in which ether exists alone without matter.'[15] Even as late as 1916 he saw only 'two ways of providing a physical basis for the quantum-theory':

[13] *L'enseignement des sciences*, éd. Liard, 1904, pp. 84, 93.

[14] AP 46 (1892) 139, 151.

[15] PRS 76A (1905) 301. *Cf.* also Jeans in *La théorie du rayonnement et les quanta*, pp. 64, 118.

> According to one view, the ether must be regarded as possessing so much substantiality that it forms an essential part of every dynamical system which is capable of emitting or absorbing radiation. . . . According to the other view, . . . the ether has not sufficient substantiality for its energy to be discussed in this way; it serves as a medium for the transfer of energy from one part of a material system to another rather than as itself being a receptacle for energy.—Jeans, *Dynamical Theory of Gases*, 1916², p. 409.

In either case the aether was regarded as active and important. But now he holds [16] that 'the paper which practically abolished the ether as a serious scientific hypothesis was published by Einstein in 1905.' Or more forcibly still:

> The luminiferous ether of Kelvin, Maxwell and Faraday, largely as the result of Einstein's new outlook on the universe, may be described as dead. It is no longer a serious scientific hypothesis, but merely an item in the unscientific jargon of popular expositions of 'wireless.'—Jeans, *Nature*, 117 (1926) 310.

Turn now to Jeans's text-book on Electricity. It is Maxwellian *sang pur*. On p. 497 we read that 'the ether transmits the action from one circuit to another,' shortly afterwards (p. 510) that 'the motion of electric charges is accompanied by a "displacement" of the surrounding medium'; even on p. 592 he refers to 'the energy stored in the ether.' But on p. 621 the unfortunate student gets a shock when he is told that 'the simpler view seems to be that there is no ether'—and therefore, he thinks, no text-book. Not at all; the adjustment is purely verbal; the equations remain; hence scientifically speaking there is no change. This is confirmed by the explanation offered:

> The relativity-theory has shown that what is essential to the ethereal explanation is not the ether but the momentum with which it was supposed to be endowed. It is quite easy to imagine a flow of momentum without there being an ether to carry it; and the conception of forces and pressures arising from a flow of momentum is one with which we have become familiar in other branches of physics, as for example the kinetic theory of gases.

So long as we keep to laboratory experiments, 'relativity' has nothing to do with the question. This momentum, supposing it is not a synonym for our old friend the 'field,' is merely a metaphor or analogy; in any case, like velocity, it must be referred to some

[16] *Nature*, 115 (1925) 362. 'The theory of relativity in effect requires that it shall be impossible to decide as to whether ether exists or not.'—Jeans, *Report on Radiation and the Quantum Theory*, 1924², p. 5.

reference-system. Jeans does not like the name 'aether,' so we offer him the term 'schesis.' The reference to particle-flow is, of course, not seriously meant; for the last thing he is prepared to do is to discuss Ritz seriously. So here we have a bulky text-book completely based on the schesis, and ending in a vehement denial of 'the ether.'

There are other examples of this curious phenomenon. Consider R. A. Houstoun's *Treatise on Light* (new edition, 1933). It is based entirely on schesic or medium propagation. But towards the end we are told (pp. 459, 467):

> It was formerly thought necessary to postulate a medium for it [light] to travel in. . . . But in this book the discussion of it has hitherto been evaded; we shall now proceed to explain why the hypothesis has been abandoned. . . . Relativity is now accepted as a faith. . . . According to Einstein every observer has his own system of space and time, i.e. his own ether. But they all have the same light-wave. It is consequently easier to abandon the conception of the ether and think of the light itself as having substance and moving through the void. It is a wave-motion without a medium.

At first it is rather a shock to the student to be told that the fundamental hypothesis of the book he has just read 'has been abandoned.' In reality nothing has happened; some irrelevant references are made to hypothetical observers, light is declared to be a 'substance,' the 'medium' is degraded to 'the void.' But not a single equation in the book is altered or withdrawn, the schesic method of propagation is still maintained as against the ballistic.

Similarly, Prof. R. W. Wood's *Physical Optics* (1911[2]) seems to be all aether up to p. 684, when he suddenly informs the reader that

> with the disappearance of the ether, we are forced to remodel our views concerning light and electromagnetic waves. . . . Light seems to be in the nature of something expelled by the source.

But the author has not the remotest intention of remodelling his equations, which include Maxwell's and assume schesic velocities. If 'our views' are quantitatively irrelevant, they are outside the scope of physics.

In *The Electromagnetic Field* of Mason and Weaver, in many ways a critical text-book, we are told on p. 324 that

> a decreasing number of physicists finds the hypothesis of an aether tenable or desirable. . . . The 'aether drift' experiments and the

> theory of relativity have removed the last excuses for continuing to assume such a substance.

And on the next page the authors, referring to Jeans, declare that

> the aether physicists seem at last to be willing, in their final philosophical paragraphs, to give up the aether; but they continue to use it throughout the body of their texts, compelling its unsubstantial texture to bear the brunt of many an argument.

This is an unexpected complaint from authors who base their own text-book on the schesis and actually give the Liénard formula. Apparently they wish merely to assert that the schesis has an 'unsubstantial texture.' But this is scientifically irrelevant; Liénard's formula says nothing about 'texture' or 'substance.'

What is our object in citing these examples? The purpose is the important one of distinguishing between the *discourse of physicists* and the quantitative formulation of physics. Once we establish this distinction we shall have acquired a technique for getting rid of such sterile discussions as those concerning the aether, the field, lines of force, dimensions, etc. We have a criterion for separating what pertains to genuine scientific physics from what pertains to the discourse—often a farrago of philosophy, paradox and imagination—in which physicists so often indulge when writing semi-philosophical or popular books and even when writing text-books. The question to be asked concerning any hypothesis is this: Does it in any way influence the measure-numbers which are to be tested by the man in the laboratory? If it does not, it may be good or bad philosophy; but it is not physics. Accordingly we have no difficulty in deciding that the hypothesis of an 'aether,' whatever it is supposed to mean, does not nowadays pertain to the science of physics; for its denial does not make any alteration in any formula. However, it is not always so easy to decide the purely scientific question which remains. Our detailed comparison of two syntheses of electromagnetics, one involving schesic velocities and the other purely relative velocity, has remained indecisive. One result clearly emerges: if we do adopt schesic velocities, the schesis must be taken as comoving with the earth,[17] i.e. as identical with the

[17] Subsequently—in the light of Michelson and Gale's optical experiment—we must add 'in its orbital motion.' The distinction is beyond the sensitivity of ordinary electromagnetics.

scientific laboratory. But obviously the debate must be adjourned to the domain of optics. This subsequent discussion—which will not be undertaken here—if it is to have scientific value and to avoid endless irrelevancies, must be confined to the formulae of physics. The word 'schesis' is doubtless a rather ugly term. But it has the advantage of being immediately connected with a fundamental formula and of being devoid of all other associations. One could not imagine a wordy warfare about the schesis; either schesic velocities occur in the formula or they do not. Whereas disputes about the existence or non-existence of 'the aether' seem to be interminable. Maxwell tells us (iv. 763) that

> it is only when we remember the extensive and mischievous influence on science which hypotheses about aethers used formerly to exercise, that we can appreciate the horror of aethers which sober-minded men had during the eighteenth century.

And perhaps, when we think of the elastic, hydrodynamical and gyromagnetic aethers of the nineteenth century, we can appreciate the horror of aethers which many sober-minded physicists feel to-day. But the schesis is neutral and innocuous; it can excite neither enthusiasm nor horror; it is therefore best adapted for objective argument.

3. 'Explanations.'

We shall now supplement the remarks on far-action which we made in Chapter VII. There is no doubt that to-day practically all of us, whether physicists or not, have a strong intuitive conviction that physical effects cannot be instantaneously transmitted through space, that they require some kind of progressive mediation. This was not always believed. 'The sun as soon as ever it appears in the east,' wrote Leonardo da Vinci,[18] 'instantly proceeds with its rays to the west. . . . The eye so soon as ever it is opened beholds all the stars of our hemisphere.' Descartes even founded his whole cosmological system on the infinite speed of light. 'I declare to you,' he wrote[19] to Beeckman in 1634 'that if this lapse of time could be observed, my whole philosophy would be completely ruined.' Römer practically settled the

[18] E. McCurdy, *Leonardo da Vinci's Notebooks*, 1906, p. 56.
[19] Descartes, *Œuvres*, ed. Tannery-Adam, i. 307.

question in 1676, before the publication of Newton's *Principia* (1687). Newton, who shared our modern conviction, was thus placed in a difficulty by his law of gravitation, which seemed to imply unmediated and instantaneous causation. He refused therefore to admit that his law proved what in the language of the time was called innate or intrinsic gravity. He wrote as follows to Bentley in 1693:

> It is inconceivable that inanimate brute matter should, without the mediation of something else which is not material, operate upon and affect other matter without mutual contact—as it must be, if gravitation in the sense of Epicurus be essential and inherent in it. And this is one reason why I desired you would not ascribe innate gravity to me. That gravity should be innate, inherent and essential to matter—so that one body may act upon another at a distance through a vacuum without the mediation of anything else by and through which their action and force may be conveyed from one to another—is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers.—*Works of Richard Bentley*, ed. A. Dyce, 3 (1838) 221 f.

As a philosopher or rather as a theologian, Newton was almost certainly in agreement with the view expressed as follows by Bentley:

> Mutual gravitation . . . is an operation or virtue or influence of distant bodies upon each other through an empty interval, without any effluvia or exhalations or other corporeal medium to convey and transmit it. This power, therefore, cannot be innate and essential to matter. And if it be not essential, it is consequently most manifest—since it doth not depend upon motion or rest or figure or position of parts, which are all the ways that matter can diversify itself—that it could never supervene to it unless impressed and infused into it by an immaterial and divine power. . . . This would be a new and invincible argument for the being of God; being a direct and positive proof that an immaterial living mind doth inform and actuate the dead matter and support the frame of the world.—Bentley, *ibid.*, p. 163.

But as a mathematical physicist he left this and other 'hypotheses' and conclusions 'to the consideration of his readers.' His views on the function of a physical theory, which have met with such unmerited obloquy, are really so apposite and modern that they deserve to be quoted at some length.

It is well known that bodies act one upon another by the attractions of gravity, magnetism and electricity. . . . What I call attraction may be performed by impulse or by some other means unknown to me. . . . We must learn from the phenomena of nature what bodies attract one another and what are the laws and properties of the attraction, before we inquire [into] the cause by which the attraction is performed.—Newton, *Opticks*, q. 31, ed. Whittaker, 1931, p. 376.

These principles I consider not as occult qualities supposed to result from the specific forms of things, but as general laws of nature by which the things themselves are formed—their truth appearing to us by phenomena though their causes be not yet discovered. . . . The unknown causes of manifest effects—such as would be the causes of magnetic and electric attractions and of fermentations, if we should suppose that these forces or actions arose from qualities unknown to us and incapable of being discovered and being made manifest—such occult qualities put a stop to the improvement of natural philosophy. . . . But to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from these manifest principles, would be a very great step in philosophy, though the causes of those principles were not yet discovered.—Newton, *Opticks*, q. 31, ed. Whittaker, 1931, p. 401.

I here use the word ' attraction ' in the general sense of any kind of endeavour of bodies to approach each other: whether this endeavour arises from the action of bodies bending mutually or agitating each other by spirits emitted, or whether it arises from the action of the aether, the air or any medium, corporeal or incorporeal, which impels contained bodies towards each other. In the same general sense I use the word ' impulse ' in this treatise to denote quantities and mathematical proportions, not kinds of forces or physical qualities. In mathematics we have to investigate the quantities of forces and the relations which follow from given conditions. Then when we come to physics we must compare these relations with phenomena. . . . And so finally we shall be able more safely to discuss the kinds of forces and the physical causes and relations.—Newton, *Principia*, lib. i, prop. 69, scholium (Glasgow, 1871, p. 188).

To understand the motion of the planets under the influence of gravity without knowing the cause of gravity, is as good a progress in philosophy [i.e. ' natural philosophy ' = physics] as to understand the frame of a clock and the dependence of the wheels upon one another, without knowing the cause of the gravity of the weight which moves the machine, is in the philosophy of clockwork.—Newton in D. Brewster's *Memoirs of the Life, Writings and Discoveries of Sir Isaac Newton*, 1855, ii. 283.

I could wish all objections were suspended [which are] taken from hypotheses or any other heads than these two : [1, That] of showing the insufficiency of experiments to determine these queries or [to]

prove any other parts of my theory, by assigning the flaws and defects in my conclusions drawn from them. Or [2, that] of producing other experiments which contradict me, if any such may seem to occur.—*Phil. Trans. abridged*, 1809, i. 735.

Having observed the heads of some great virtuosos to run much upon hypotheses as if my discourses wanted a hypothesis to explain them by, and [having] found that some, when I could not make them take my meaning when I spoke . . . abstractedly, have readily apprehended it when I illustrated my discourse with an hypothesis: for this reason I have here thought fit to send you a description of the circumstances of this hypothesis, as much tending to the illustration of the papers I herewith send you. And though I shall not assume either this or any other hypothesis, . . . I shall sometimes, to avoid circumlocution and to represent it more conveniently, speak as if I assumed it and propounded it to be believed. —Newton to Oldenburg (1675), Brewster, i. 136, 391.

These quotations make it abundantly clear that Newton's standpoint was identical with that maintained in this book. We must learn the laws and properties of electromagnetic actions before we start inquiring into causes; we can find these laws though the causes be not yet discovered. This would be a great step in physics, which is a practical affair comparable to the art of a clockmaker. Occult qualities are only a hindrance. The sole test of a physical theory is its ability to coordinate experimental results. But it is observed that the heads of some great virtuosos run much upon hypotheses; there are many indeed who cannot grasp physics when expounded abstractedly but who readily apprehend it when the discourse is illustrated with hypotheses. For them let lines of force, fields, aether and the like be provided. Sometimes, to avoid circumlocution and to facilitate representation, these extraneous pictures and models are described as if they were propounded to be believed. But this is merely a pedagogical concession, irrelevant to the logical structure and pragmatic efficiency of physics. This is the Newtonian outlook here defended. As an old writer puts it,

he only who in physics reasons from phenomena, rejecting all feigned hypotheses, and pursues this method inviolably to the best of his power, endeavours to follow the steps of Sir Isaac Newton and very justly declares that he is a Newtonian philosopher.—W. J. s'Gravesande, *Mathematical Elements of Natural Philosophy*, London, 1747[6], i. p. viii.

We are not here concerned with the law of gravitation, to which Newton adhered as a good law, still very approximately

valid, in spite of the fact that he could give no 'cause' for it and that it seemed to violate a cardinal physical intuition. Our two alternative generalised Newtonian laws for electromagnetics do not suffer from this latter disability, for they both imply propagation in time. Once more we point out how one of these laws (Ritz's) is completely ignored, even as a theoretical alternative, in contemporary expositions.

> Faraday and Maxwell replaced Coulomb's law of force between electrified particles . . . by a propagation of influences between contiguous particles of a universal medium; and since then the fortunes of action-at-a-distance have steadily declined.—E. T. Whittaker, preface to *Newton's Opticks*, 1931, p. ix f.
>
> Every causal action propagates itself from point to point with a finite velocity through space. . . . There have been several different theories of action at a distance in electrodynamics, but only one of contiguous action, namely, that of Maxwell.—Planck, p. 1 f.

In this matter present-day writers out-Maxwell Maxwell. For he fully admitted the two alternatives.

> The mathematical expressions for electrodynamic action led, in the mind of Gauss, to the conviction that a theory of the propagation of electric action in time would be found to be the very keystone of electrodynamics. Now we are unable to conceive of propagation in time, except either as the flight of a material substance through space or as the propagation of a condition of motion or stress in a medium already existing in space. . . . In fact, whenever energy is transmitted from one body to another in time, there must be a medium or substance in which the energy exists after it leaves one body and before it reaches the other. . . . If we admit this medium as an hypothesis, I think it ought to occupy a prominent place in our investigations and that we ought to endeavour to construct a mental representation of all the details of its action; and this has been my constant aim in this treatise.—Maxwell, ii. 493 (§ 866).

That is, Maxwell admitted that energy could travel either in a medium or in a projectile. Or, to put the matter more generally, the force-law might involve either schesic velocities as in a medium or relative velocity as in ballistics. But Maxwell was not content with adopting the former alternative. His 'constant aim' was to construct 'a mental representation,' a kind of mechanical model. Now, even if he had succeeded, would he thereby have really explained anything? He himself had doubts at times.

> The action between the magnets is commonly spoken of as an action at a distance. Attempts have been made, with a certain

amount of success, to analyse this action at a distance into a continuous distribution of stress in an invisible medium, and thus to establish an analogy between the magnetic action and the action of a spring or rope in transmitting force. But still the general fact that strains or changes of configuration are accompanied by stresses or internal forces, and that thereby energy is stored up in the system so strained, remains an ultimate fact which has not yet been explained as the result of any more fundamental principle.—Maxwell, vii. 66 f.

If we explain magnetism by means of the analogy of a spring or a rope, wherewith shall we explain the spring and the rope? The whole modern tendency is to work the other way, to cherish the pious hope of explaining the action of springs, ropes and other things by means of electromagnetism. At any rate Maxwell's way is admittedly a failure.

At no time since electric phenomena have become part of science has any formula been devised which might in any way pass as a mechanical theory consistent with these phenomena.—E. Meyerson, *Identity and Reality*, 1930, p. 64.

Are then the electromagnetic processes thus referred back to mechanical processes? By no means; for the vector A employed here is certainly not a mechanical quantity.—Planck, *Eight Lectures on Theoretical Physics*, 1915, p. 111.

We may define the mechanical conception of Nature as the view that all physical phenomena can be completely reduced to movements of invariable and similar particles. . . . I will now consider the real stumbling-block of the mechanical theory, namely light-ether. . . . All these attempts were fruitless; light-ether mocked all efforts to explain it mechanically.—Planck, *A Survey of Physics*, 1925, pp. 43, 51.

The failure of all attempts at a mechanical explanation of electromagnetic phenomena has led to the present abandonment of the aether-hypothesis.—Jouguet, p. 128.

As regards the nature of the luminiferous ether, we are still confined to vague speculations.—Nernst, *Theoretical Chemistry*, 1923, p. 465.

Neither Maxwell nor his successors succeeded in thinking out a mechanical model for the ether. . . . Almost imperceptibly theoretical physicists adapted themselves to this state of affairs. . . . Whereas they had formerly demanded of an ultimate theory that it should be based upon fundamental concepts of a purely mechanical kind—e.g. mass-densities, velocities, deformations, forces of gravitation—they gradually became accustomed to admitting electric and magnetic field-strength as fundamental concepts alongside of the mechanical ones, without insisting upon a mechanical interpretation of them. The purely mechanistic view of nature was thus abandoned.—Einstein, *The World as I see It*, 1935, p. 195.

These and similar conclusions are quite unwarranted. Even if we shut our eyes to Ritz's alternative and accept Liénard's force-formula as something not further explicable at least in terms of molar mechanics, we do not thereby abandon schesic velocities; we actually affirm them. We merely abandon the elastic-solid aether. Nor are we for one moment compelled to admit new fundamental concepts, as Einstein affirms. In a very real sense we retain the usual mechanical quantities; nothing else occurs in Liénard's formula, for even the measure called 'charge' is defined mechanically. So, in spite of Planck's assertion, even the vector-potential can legitimately and intelligibly be described as mechanical. But we confess that we cannot fit into the current schemes of elasticity and hydrodynamics the particular collocation of measures which occurs in Liénard's formula for force. And, when we come to think of it, we are unable to see any reason why we should have succeeded. Deprived thus of their cherished 'hypothesis' as Newton would call it, having lost the aether of cogs and pulleys, physicists found a new outlet for their ineradicable tendency to concretise and substantialise. And so the 'field' took the place of the 'aether,' auxiliary vectors like **E** and **H** were made to strut about in their own right. The more it changes, the more it is the same thing.

These psychological subterfuges are unavailing. We are face to face with what appears to be a mysterious formula. But things are not quite as bad as Prof. W. L. Bragg depicts (p. 102):

> Magnetic forces, and the relations between electrical currents and magnetic fields, are as mysterious and unlike any mechanical forces as they can well be. For this reason they are difficult to follow, and it is hard to get a conception of them into one's head. . . . It must be admitted that they cannot be explained, but must be accepted as part of the fundamental behaviour of all things.

It is not the whole congeries of the phenomena of currents and magnetic fields that is inexplicable. For we can coordinate and 'explain' all these as statistical results of the force-formula. It is to this alone that we must confine the 'mystery.' We cannot 'explain' everything, we must start somewhere. And if we are asked to 'explain' the fundamental force-formula, we can only say that it works, that it explains other things. 'Why do electrified particles obey these laws?' asks Bragg (p. 44). 'At

this stage,' he answers, 'we are reduced to saying in an exasperated way: *Because they jolly well do!*' Or in less expressive language:

> Though many attempts have been made to explain it, the ultimate nature of electric force remains unknown to us. The behaviour of bodies of appreciable size can be explained in terms of the sub-atomic particles making them up; but it is at present impossible to say why these sub-atomic particles act upon one another as they do. All that the explanation has achieved is to remove the difficulty one step further back.—J. Pilley, p. 5.

There is, of course, nothing very novel in this point of view, which has been gaining increasing acceptance from physicists, though they do not always carry out its logical consequences.

> It is now a full quarter of a century since physical science, largely under the leadership of Poincaré [what about Duhem ?], left off trying to explain phenomena and resigned itself merely to describing them in the simplest way possible.—Jeans, *The Universe Around Us*, 1929, p. 329.

Whatever about physical science itself, it is not at all clear that physicists have yet 'left off' trying to turn physics into philosophy. Sir James Jeans himself is hardly a satisfactory example of this self-denying ordinance of physics, which we have just heard him announcing.

> The twentieth-century physicist is hammering out a new philosophy for himself. . . . The whole of physical nature follows us about like a rainbow or like our own shadow.—Jeans, *New Background of Science*, 1933, pp. 2, 109.
>
> [Science] came to recognise that its only proper objects of study were the sensations. . . . The dictum *esse est percipi* was adopted wholeheartedly from philosophy. . . . Those who did not adopt it were simply left behind, and the torch of knowledge was carried onward by those who did.—Jeans, *Philosophy*, 7 (1932) 11.

Obviously there are two Jeans's: Jeans the scientific follower of Newton, and Jeans the philosophical adherent of Berkeley. It is not always easy to dissociate this dual personality.

4. The 'Field.'

In spite of the fact that, as regards the range of phenomena considered in this book, the force-formula is our ultimate element of *scientific description*, numerous further 'explanations' abound. The type of mind which found satisfaction in mechanical aether-models is by no means extinct; it has merely shifted its vocabulary

from engineering to pure mathematics. There must be some reason for this tenacious persistence. This reason can easily be inferred from the fact that the 'explanations' chiefly occur in the 'discourse' of contemporary physicists, and especially in the philosophical and theological treatises which they insist on publishing. We feel 'in our bones' that the problem is not ended by using a Greek word such as schesic or ballistic; we are convinced that there must be 'something behind' the force-formula. We feel we must choose between some spatially extended framework (medium) and projected emanations. In other words, we experience the desire to provide an ontological basis for our formula. This instinct is by no means an unintelligent impulse; for we cannot be satisfied with 'space' which is not a real entity at all.[20] But we are not now considering these philosophical problems at all. The issue is whether, within the range of scientific physics, there is any further 'explanation.' Inasmuch as physics deals only with measure-numbers, this must mean: whether there is any simpler and more synthetic formula from which we can derive that of Liénard or Ritz. There are, of course, the considerations which led to these formulae. But these are not really deductions, they are rather tentative *a posteriori* correlations and syntheses of experimental results; they throw no light whatever on the final formula. Every attempt to deduce these formulae, to base them on some more evident mechanism, has hopelessly failed. No further scientific explanation, or rather description, seems possible.

The contrary idea however is widely prevalent and assiduously cultivated. It seems to be chiefly based on the delusion that the science of physics can be extended by terminological alterations and rhetorical gymnastics. A good deal of what is miscalled modern physics, at least in discursive semi-philosophical works, is really nothing but verbal adjustments which do not result in changing one iota of any existing formula. What is algebraically ineffective must be regarded as scientifically irrelevant. Another characteristic of the new phraseology is its uncritical acceptance of the older Maxwellian ideas, which lend themselves so readily to the manipulation of mathematics and the manufacture of paradox. What is entirely lacking is any serious attempt to

[20] Space is *ens rationis cum fundamento in re.* Newton thought that space was *ens reale*; Clarke, carrying this idea further, identified it with God's immensity.

adjust statements to the electron theory. Philosophical physics is as much out of date as most of our text-books. Prof. Soddy's complaint is justified :

> At the present time, when so much of our theory is merely a transitional patchwork of new ideas upon old habits of thought rather than any consistent substitute for the old way of regarding things, surely *all* these old ideas ought to be critically examined, and, in accordance with the modern tenets, nothing allowed to be assumed which is not directly amenable to observation. . . . Mere familiarity and reiteration of ideas is taking the place of genuine theoretical advance amenable to scientific proof.—F. Soddy, *The Interpretation of the Atom*, 1932, p. 341.

Let us begin our examination of these so-called explanations with Faraday's lines of force as advocated in the following quotations.

> Instead of an intangible action at a distance between two electrified bodies, Faraday regarded the whole space between the bodies as full of stretched mutually repellent springs.—J. J. Thomson, xi. 10.
>
> The conception of lines of force is in my opinion one of the greatest of Faraday's many great services to Science.—J. J. Thomson, iv. 29.
>
> The view increasingly plausible [is that] according to which a real existence is ascribed, exactly as was done by Faraday, to the individual lines of force, in the sense that they exist in a certain measure as separate entities.—Grimsehl-Tomaschek, p. 50.
>
> As a result of the researches of Faraday and Maxwell we regard the properties of charged bodies as due to lines of force which spread out from the bodies into the surrounding medium.—E. W. Barnes, *Scientific Theory and Religion*, 1933, p. 195.
>
> Nothing has proved of greater importance and more fruitful than this conception of lines of force. . . . [It is] a fundamental advance for all time.—Lenard, v. 255, 257.

Concerning electricity itself Faraday was reserved and vague :

> We are in . . . ignorance of electricity, so as to be unable to say whether it is a particular matter or matters or mere motion of ordinary matter or some third kind of power or agent.—Faraday, § 852 (i. 249).
>
> Whether there are two fluids or one or any fluid of electricity, or such a thing as may be rightly called a current, I do not know.—Faraday, § 3249 (iii. 410).

But he believed in 'the possible and probable physical existence' (iii. 438) of lines of force for gravitation, electrostatics, magnetism. This idea he considered compatible with 'the emission and the

aether theories' and with 'the idea of a fluid or of two fluids' (iii. 330). He indulged in the speculation that light and radiant heat were tremors of the lines of force: 'a notion which, as far as it is admitted, will dispense with the ether, which in another view is supposed to be the medium in which these vibrations take place' (iii. 447). In fact they supersede both matter and aether:

> I do not perceive in any part of space, whether (to use the common phrase) vacant or filled with matter, anything but forces and the lines in which they are exerted.—Faraday, iii. 450.
>
> Space must be a conductor, or else the metals could not conduct. . . . I feel great difficulty in the conception of atoms of matter . . . with intervening space not occupied by atoms. . . . [On this view] matter is everywhere present, . . . matter will be continuous throughout.—Faraday, ii. 286, 289, 291.

It will be observed therefore that Faraday's Boscovichian idea of centres and lines of force was based on a non-atomistic conception of matter and electricity. Had Maxwell accepted the atomistic view of electricity, he would hardly have been so enthusiastic for Faraday's ideas. On the other hand, if he had developed them logically, he would not have invented his 'displacement.'

> What Maxwell called the electric displacement in any direction at a point is the number of Faraday tubes which pass through a unit area through the point drawn at right angles to that direction, the number being reckoned algebraically. . . . For my own part, I have found the conception of Faraday tubes to lend itself much more readily to the formation of a mental picture of the processes going on in the electric field than that of electric displacement, and have for many years abandoned the latter method.—J. J. Thomson, xi. 15 f.

This dispute about methods really pertains to pedagogy, not to physics; it belongs to the educational psychology of 'mental pictures.' This geometrical phraseology is adapted to the capacity of those who are unable to understand elementary vector analysis. It is often of general convenience as a graphical representation, like parallels of latitude and meridians on the earth's surface. The utilisation—still less, the substantialisation—of such conventions does not add one iota to knowledge. Faraday's lines of force are not so much a 'service to science' as a contribution to educational method. The physical hypotheses on which he based them are, of course, superseded. The position to-day may be fairly summed up as follows:

> Any explanation of this kind which attributes mechanical properties to tubes of force is highly artificial as there is no evidence for their existence. Nowadays physicists are becoming more and more inclined to shun such explanations ; so the mechanical explanation of the interaction of electrically charged bodies is rapidly falling into disuse. It still lingers in text-books, however, and it is important to recognise its arbitrariness.—J. Pilley, p. 94.

There is no doubt that lines and tubes of force are merely a geometrical and intuitive representation of a vector or scalar whose value depends on its position. Maxwell himself wrote in 1855 (viii. 711) :

> I have been planning and partly executing a system of propositions about lines of force, etc., which may be *afterwards* applied to electricity, heat or magnetism or galvanism, but which is in itself a collection of purely geometrical truths embodied in geometrical conceptions of lines, surfaces, etc. The first part of my design is to prove by popular—that is, not professedly symbolic—reasoning, the most important propositions about V and about the solution of [Laplace's] equation and to trace the lines of force and surfaces of equal V.

The very wording of this letter shows clearly that we are concerned solely with 'a collection of purely geometrical truths' applicable to such different subjects as electricity, magnetism and heat. The idea of lines and tubes is merely a translation of the analytical structure of quantities which occur with very different meanings in diverse branches of physics. This is sometimes helpful in the same way in which a graph helps us to realise the significance of an equation. It is especially useful for those who are weak in mathematics.

> The object of this paper is to endeavour to supply a method of representing in terms of physical conceptions the processes occurring in physical phenomena. It is an attempt to help those who like to supplement a purely analytical treatment of physical problems by one which enables them to visualise physical processes as the working of a model, who like in short to reason by means of images as well as by symbols.—J. J. Thomson, xv. 679.

The method, however, becomes objectionable when it substitutes clumsy geometrical arguments for simple analytical ones, for it then ceases to be a psychological aid. It becomes still more objectionable when writers proceed to endow their expedients with substantiality, to speak of their graphical representations as if they were threads, tubes or springs which somehow 'explain' the equations from which they were derived.

The idea of moving lines of electric force or Faraday tubes has been used in brilliant fashion by Sir Joseph Thomson to describe the processes which take place in an electromagnetic field. Scientists are still undecided whether to regard the lines of force as physical realities or merely as useful mathematical tools.—H. Bateman, PM 34 (1917) 405.

The only important theory which has ever been proposed to explain the properties of electricity is that of Faraday. . . . Faraday supposed that the region surrounding charged bodies was traversed by 'lines of force' . . . analogous to elastic strings.—N. Campbell, iii. 6.

According to the older mathematical theory of electromagnetism (mainly due to Ampère) the existence of a magnetic field is inseparably connected with the motion of charged bodies or at least of charges; there is nothing in the theory to suggest that there could be a magnetic field apart from the motion of charges. But according to Faraday the magnetic field is associated, not with the motion of charges, but with the motion of the tubes attached to them; and since the tubes are flexible, there is every reason to suppose that the tubes might move without the charges.—*Ibid.*, p. 12.

This is certainly a good example of the self-hallucination induced by one's own vocabulary. We obtain the expression for the force-vector at any point. We then draw the tangent-lines of this vector as a useful graph. To strengthen our belief we call them 'tubes.' Next we endow them with 'flexibility' and declare that they 'might move without the charges' from which we started. Then we pick out one portion of the force on moving charges and call it 'magnetic force' and similarly attribute to it flexible independently moving tubes. Finally, we declare that this double system of tubes is 'the only important theory which has ever been proposed to explain' electricity, magnetism, and light. This tube-theory is a most ingenious specimen of soporific phraseology.

We next proceed to an investigation of the term 'field,' on which such emphasis is laid nowadays. So many far-reaching conclusions have been based on this term, that it will be necessary to give extensive and representative quotations. Let us begin with its apparently innocuous introduction.[21]

[21] Similar legerdemain occurs elsewhere in physics. "It is astonishing to find such a grandiose structure as the mathematics of the quantum theory ostensibly based upon a mere restatement of a definition."—K. T. Darrow, *Rev. Sci. Instr.* 7 (1936) 377.

The electric field is the portion of space in the neighbourhood of electrified bodies, considered with reference to electric phenomena—Maxwell, i. 47.

The neighbourhood of a magnet is for convenience called a magnetic field.—Maxwell and Jenkin, p. 63.

The space all around a magnet pervaded by the magnetic forces is termed the field of that magnet.—S. P. Thompson, p. 106.

The space in and around a given system of charges is called the electric field of those charges.—Livens, ii. 7.

The space within which the aether is sensibly disturbed and within which sensible ponderomotive forces are exercised, . . . is called the electrostatic field.—Drude, iii. 247.

The space within which these Faraday tensions manifest themselves is called the field.—Schaefer, i. 11.

What is there in the space between the plates ? The physicist replies : an electric field.—Pohl, p. 22.

There is said to be an ' electric field ' in a region which is traversed by lines of force.—Bragg, p. 28.

The space in the neighbourhood of charged bodies is called an ' electric field.' . . . E denotes the force per unit charge at any point of the field ; it is called the electric intensity or the electric vector or simply the field.—Ramsey, pp. 11, 16.

We call the space around a heavy mass, an electrified body or a magnet, in which the effects of gravitation, etc., can be shown to exist, its *field* (gravitational, electrical, magnetic). . . . [Faraday's] concept of a field of force has fundamentally changed our whole world-picture.—B. Bavink, *The Anatomy of Science*, 1932, p. 110 f.

Just ' for convenience ' the ' space ' round a planet, a charge or a magnet is ' called ' a ' field ' ; or a ' field ' is ' said to be ' ' in ' the space. So far this is mere conventional shorthand. But when the astonishing claim is made that this harmless terminology has fundamentally changed our whole world-picture, it behoves us to move warily. It may be the first step that is important. The word ' space ' evokes criticism. It cannot mean imaginary space without boundary ; it must implicitly contain a reference to some framework which is either material or determinable relatively to ordinary objects. We cannot help asking whether, when a body—gravitational, charged or magnetic—moves in any manner relatively to a laboratory, its ' space ' moves with it. More simply still, if a charge is at rest in a laboratory, is its ' space ' also at rest in the laboratory ? If we wished to be very inquisitive, we might even ask whether an *isolated* charge has a ' space ' at all. There are all kinds of ambiguities and problems concealed in that word ' space ' ; its occurrence in a work on physics should always be a danger-

signal warning us to sharpen our critical faculty. How little do students, plotting the field of force of a couple of point-charges (as illustrated in elementary books) or watching iron-filings arranging themselves round a current or a magnet, realise that they are helping to produce a fundamental change in the modern man's outlook on the world!

Let us try to trace this growth of a simple phrase, familiar to a schoolboy in a laboratory, into a new *Weltanschauung* upsetting our philosophy and theology. We start with the elementary law that the attraction (on unit mass) of a particle m is $\mathbf{F} = \gamma m \mathbf{r}_1/r^2$, or that the force exerted by a point-charge e on another e'—both being 'at rest,' however we interpret that—is $e'\mathbf{E} = ee'\mathbf{r}_1/\alpha r^2$. There is no dispute concerning the starting-point. The next step is the assertion that $\mathbf{E}$ exists even when there is no e', and of course $\mathbf{F}$ when there is no unit mass at the point; not to mention $\mathbf{H}$ at the point P when there is no magnet or 'magnetic pole' at P.

In defining a magnetic field as a region possessing or possessed of a peculiarity, he cautioned us against thinking that a field of the strength of so many c.g.s. units meant so many dynes; the field is 'there,' whether or not an isolated unit magnetic pole be set in it to experience the force of so many dynes; the dynes came in by reason of our arbitrary choice of a special aspect of the peculiarity of the field.—H. F. Newall with reference to Maxwell's lectures in 1878, in *History of the Cavendish Laboratory*, 1910, p. 106 f.

In the near-action theory the field-strength is a reality which exists even when the reacting bodies are removed.—P. Hertz, i. 78.

Maxwell's theory then goes on to ascribe to this vector $\mathbf{E}$ a self-existent reality independent of the presence of a testing body.—Abraham-Becker, p. 55.

$\mathbf{E}$ is not the actual strength of the electric field at the point where the charge e is situated, but rather the field-strength that would exist at that point if the charge e were not present at all.—Planck, p. 110.

We must suppose that it [E] exists at all points about q even when our test charge is not present; but we can prove its existence only by bringing the test charge to Q.—White, p. 3.

Following Faraday and Maxwell, we regard the space surrounding a magnetic body as a 'magnetic force-field,' which on its part exerts forces on magnets which exist in it.—Fürth, p. 289.

The plausibility of this assertion lies entirely in our ontological preposession that there must be some objective condition at P through which it results that, if a mass, charge or 'magnetic

pole' is placed at P, it experiences a mechanical force. This conviction is by no means denied by physics. But it calls for certain comments.

(1) The point P must be defined with reference to scientifically ascertainable axes. According to Liénard's formula, for example, the point must be at rest in the schesis; according to Ritz it is at rest relatively to the central charge e.

(2) The assertion, taken by itself apart from the quantitative force-law, is scientifically otiose. It is merely the physically irrelevant statement of a metaphysical conviction. 'A physical theory,' say Mason and Weaver (p. 73), 'has no concern with "conditions" at an empty point in space, and for the precise reason that the point is empty.' It is certainly difficult to believe that this idea of 'field' is anything but a useless metaphor when it is rejected as scientifically irrelevant even in elementary textbooks:

> Any statement which is made about the electric field in the neighbourhood of a charged body cannot strictly speaking be taken to mean more than that a second charged body, if placed there, would behave in a particular way. . . . The physical reality of the magnetic field remains as questionable as that of the electric field.—J. Pilley, pp. 84, 198.

(3) While it is quite credible and plausible that this objective condition or cause 'would exist at that point if the charge e' were not present at all,' it is rather incredible that **E** which, as we shall see in the next chapter, is a pure number should 'exist' there in the absence of the sole experiment which defines it, that it should be 'a self-existent reality independent of the presence of a testing body.' This is certainly not a legitimate physical theory at all; it is the confusion of metaphysical belief with metrical physics.

(4) The separate co-existence of **E** and **H** does not follow at all; it certainly is not a characteristic of near-action theories, of which Ritz's is one. The expression 'electromagnetic' field is much more objectionable, if interpreted ontologically, than 'electric field,' for it implies the existence of both kinds of 'force.'

(5) The 'field' may act as a metaphysical background, but it certainly does not act as a scientifically verifiable physical intermediary.

Physical measurement is confined to the investigation of the effect of one charged particle on the motion of another. Electro-

magnetic theory, however, describes the effect through the agency of an intermediate concept—the electromagnetic field.—Leigh Page, x. 221.

This particular form of theory does not and could not exalt this 'concept' into being a physical agent, something mensurationally detectable in a laboratory; it merely uses certain auxiliary vectors—just as it uses potential-waves—in the mathematical elaboration of the final result.

Let us illustrate these points by a quotation:

> We shall now suppose that the space surrounding an electric charge is different from that elsewhere. We do not need to consider how this is brought about. It may be that the charge produces a change in the state of the surrounding aether; or the charge may have parts which extend into the region about it; or it may be merely a manifestation of a hyperspatial mechanism; or it may even be something which is incapable of description in mechanical terms. The important point is that if another charge is placed at any point of such space it will be acted on by a force and accelerated. —Richardson, p. 14.

The cause may be all kinds of things, some of them rather queer; but we do not need to consider how it is brought about; in fact we have not got the faintest notion. The important point is that another charge if placed at the point would be acted upon by a force. It is not merely the important point; so far as physics is concerned, it is the only point. There is therefore scientific—we prescind from philosophical—justification for this severe criticism passed by Prof. Bridgman (i. 57 f., 136):

> The reality of the field is self-consciously inculcated in our elementary teaching, often with considerable difficulty for the student. This view is usually credited to Faraday and is considered the most fundamental concept of all modern electrical theory. Yet in spite of this I believe that a critical examination will show that the ascription of physical reality to the electric field is entirely without justification. I cannot find a single physical phenomenon or a single physical operation by which evidence of the existence of the field may be obtained independent[ly] of the operations which entered into the definition. . . . I do not believe that the additional implication of physical reality has justified itself by bringing to light a single positive result, or can offer more than the pragmatic plea of having stimulated a large number of experiments, all with persistently negative results. . . .
>
> The electromagnetic field itself is an invention and is never subject to direct observation. What we observe are material bodies with

or without charges—including eventually in this category electrons—their positions, motions and the forces to which they are subject.

By way of contrast to this unanswerable criticism, let us look at one of those typical pronouncements of what is often called modern physics.

> The notion of the *field* is a fundamental concept of modern physics. We live all our brief days in a 'field,' which we do not notice merely because we are accustomed to living in it. . . . This magnet, innocent as it looks, has by its mere presence influenced and transformed the space around it. It has thrown the space in its neighbourhood into a peculiar state of tension. . . . For Faraday the field was something physically actual, something real, and indeed the only fundamental thing. The field alone is important; the charges are significant only in so far as they build up the field, throwing nothingness into that peculiar state of tension. It called for unimaginable courage thus to liberate electricity from the fetters of matter and translate it into empty space.—P. Karlson, *You and the Universe*, 1936, pp. 79, 82 f, 85 f.

Here, unlike the Greek legend of Kronos devouring his children, the field starts to devour its parent-charge. The field started as the humble offspring, the shadowy penumbra surrounding a charge; it ends by destroying not only electricity but matter! 'According to our present conceptions,' says Einstein,[22] 'the elementary particles of matter are in their essence nothing else than condensations of the electromagnetic field.' A statement which certainly calls for 'unimaginable courage'! We start with a point-charge or electron e, something which we admit constitutes currents and beta-rays or which hits a Geiger counter. Next we write down its field-strength $E = e/r^2$, i.e. the force which a unit charge would experience if it were placed there; then we integrate something like $E^2/8\pi$ over infinite space—outside the point-charge itself; this is supposed to give us the energy—of the point-charge. Or we just say that 'the electric field in free space' is, or has replaced, the electron. This looks uncommonly like a gratuitous duplication. Read the following, for instance.

> Faraday noticed that not only the metal ball but also the space around it must be regarded as filled with electric substance. For electrical effects can be detected in this neighbourhood. . . . If we are to understand this, we can but assume a passage of electric force through free space to the point where the effects of attraction are

[22] *Sidelights on Relativity*, 1922, p. 22.

> observed. Faraday named the state of electrical force in free space an 'electric field.' Electricity accordingly exists in two entirely different forms: the electric substance within the conductor and the electric field in free space. Faraday's conceptions were given the form of a mathematical theory by his compatriot Clerk Maxwell . . . and give the key to an unforeseen understanding of virtually all electrical phenomena.—Reichenbach, *Atom and Cosmos*, 1932, p. 124 f.

This, so far from being modern, is really quite out of date. It seems to be forgotten that Faraday and Maxwell were entirely opposed to our present atomistic view of electricity. Many present-day writers seem still to be imbued with the same bias. The author just quoted tells us later on that 'we'

> usually think of electricity, not as a substance but rather as a condition or an aggregate of forces. We must, however, reconcile ourselves to the fact that the substance-atom conception of electricity has led to such successes that it can no longer be earnestly doubted to-day.—Reichenbach, *ibid.*, p. 187.

So eventually he reconciles himself to the electron theory which can no longer be seriously doubted. What then becomes of the 'two entirely different forms' of electricity, the charge itself and its field? Apparently one 'form' is kept for laboratory use and for theoretical expositions such as is contained in this book; we are all electronists for such purposes. The other 'form' is kept for general pronouncements about the nature of the physical universe.

> It is now generally realised that the electromagnetic field, with its singularities the electric charges, is the fundamental entity in terms of which phenomena are to be explained.—H. A. Wilson, iii. 191.
>
> The fundamental concept of present-day electric and magnetic theory is the 'field,' not the 'charge' or 'pole.' . . . We must therefore realise quite clearly that concepts occurring in electromagnetism—such as field, line of force, etc.—are fundamental concepts in their own right, which can equally well be regarded as the bases for explaining all kinds of other notions such as the customary mechanical concepts.—B. Bavink, *Science and God*, 1933, p. 34.

We have at this stage reached the full evolution of 'field.' Its lowly birth in iron filings and test-charges would never lead one to anticipate that it would finish its career as the fundamental entity in terms of which all phenomena are to be explained. The extraordinary feature of this evolution is that it has been purely verbal, a gradual and subtle enlargement of the *discourse* of

physicists, as if they were carried away by their own eloquence. The laboratory has become quite forgotten.

And now let us return to the 'aether.'

We merely use the word 'aether' as a convenient means of describing those properties of space which are concerned in electromagnetic phenomena.—Livens, ii. 236.

In recent times the idea of the aether has taken on a much vaguer and more satisfactory shape, merely as space endowed with certain properties. . . . With the proviso that aether merely is a convenient noun to describe the properties of space, it is often a convenient grammatical construction to have.—C. G. Darwin, *The New Conceptions of Matter*, 1931, p. 23.

Space as we know it has these geometrical and physical properties which cannot be separated; and so we now merely regard them as properties of empty space and do not introduce the idea [name ?] of an ether. . . . So-called empty space is not nothing; it has properties and so must be something. . . . But it is not material; matter consists of electrons and protons, and there are none of these in empty space.—H. A. Wilson, iv. 34 f.

Since aether is not material it has not any of the usual characteristics of matter—mass, rigidity, etc.—but it has quite definite properties of its own. We describe the state of the aether by symbols, and its characteristic properties by the mathematical equations that the symbols obey. There is no space without aether and no aether which does not occupy space. Some distinguished physicists maintain that modern theories no longer require an aether, that the aether has been abolished. I think all they mean is that, since we never have to do with space and aether separately, we can make one word serve for both; and the word they prefer is 'space.' I suppose they consider that the word aether is still liable to convey the idea of something material. . . . Those to whom the word space conveys the idea of characterless void are probably more numerous than those to whom the word aether conveys the idea of a material jelly; so that aether would seem to be the less objectionable term. But it is possible to compromise by using the term 'field.'—Eddington, *New Pathways in Science*, 1935, p. 39.

To give up the notion of an ether will be very hard for many physicists. . . . Consideration will show us, however, that by giving up the ether we have done nothing to destroy the periodic or polarisable nature of a light-disturbance . . . passing through a given point in space. . . . There is no need of going beyond these actual experimental facts and introducing any hypothetical medium.—Tolman, *Relativity of Motion*, 1917, p. 175.

An electromagnetic field is a physical condition which is propagated throughout space with a finite velocity in accordance with laws expressed by Maxwell's equations.—V. Lenzen, *Monist*, 41 (1931) 487.

> The electromagnetic field consists of ' something'—the physicist cannot say more—which satisfies Maxwell's equations.—B. Russell, *Mind*, 31 (1922) 478.

Observe the extraordinary position. Physics, which is supposed to be a metrical science, has now to be discussed with reference to the feelings and taste of physicists. The word ' aether ' may suggest a material jelly. Of course it ' will be very hard for many physicists ' to give up the notion ; but they are consoled and helped by sympathetic colleagues. Some of them apologise by explaining that they use ' aether ' only as a conveniently vague noun for describing the properties of ' space.' But others think that ' space ' smacks too much of the characterless void. So ' field ' is proposed as a brilliant compromise.

Now these verbal adjustments are of no possible relevance or interest to the science of physics, unless they are connected or correlated with changes in equations and formulae. If Prof. A, Prof. B and Prof. C keep unaltered the Maxwell-Hertz equations, as in actual fact they do, then it is a sheer waste of time to be debating what term they will apply to what we have called by the ugly neutral name of ' schesis.' It is not only a waste of time, it is positively misleading ; for the general public, who are sparingly provided with formulae, are inevitably led to believe that this transformation of vocabulary is due to some advance in theory or experiment. Whereas in the domain of electromagnetics—with which alone we are here concerned—these writers have not budged beyond the era of Maxwell.

We shall now make some comments on the foregoing quotations.

(1) Maxwell's equations, as well as Liénard's formula, imply a schesis or framework. Its sole scientific purpose is to provide the necessary kinematic basis for the velocities which occur in the solution (the retarded potentials).

(2) The so-called electromagnetic field consists of the vectors E and H, which are measure-numbers experimentally ascertainable at least in the combination $\mathbf{F} = \mathbf{E} + c^{-1}V\mathbf{v}\mathbf{H}$.

(3) *Experimental* physics cannot deal with differential equations, but only with their solution. We have already shown that the accepted solution, which goes beyond the equations, results in Liénard's force-formula. The differential equations themselves are relegated to being part of the preliminary mathematical manipulation.

(4) It is untrue to say that vectors such as **E** and **H** are

'properties of space,' 'characteristic properties' of the kinematic framework; that they are a propagable 'physical condition'; that they are an unknowable 'something'; that they 'describe the state of the aether.'

(5) The word 'space' is highly ambiguous and misleading. The expression 'there is no space without aether and no aether without space,' has implications which are objectionable or at least unnecessary. We do not know if the schesis 'occupies' space; from the scientific viewpoint there are many occupable 'spaces.' It is a matter for experiment to determine whether there are one or more 'aethers' or frameworks. 'A given point in space' means a point determined with respect to the schesis. Propagation 'throughout space' means propagation relatively to the schesis. In such phrases 'space' seems to imply that in laboratory experiments the schesis is at rest relatively to the fixed stars; whereas all electromagnetic results prove that, if there be such a schesis at all, it must move with the earth in its (orbital) motion.

We shall now give a series of further quotations, as they relate to another important point connected with this debate on the existence or non-existence of the aether.

> An electromagnetic wave is not a mechanical oscillation but a periodic change in the field. . . . Anyone who fails to get clear about this point will never understand modern physics.—B. Bavink, *Science and God*, 1933, p. 35.
>
> Light-waves were transformed into periodic vibrations of an electromagnetic field in empty space.—Frenkel, *Wave-Mechanics: Elementary Theory*, 1932, p. 6.
>
> The idea of a field existing in empty space and not requiring a medium to sustain it, gradually began to win ground.—H. Weyl, *Space—Time—Matter*, 1922, p. 169.
>
> To-day there is no physicist who does not know that light is nothing but a periodically changing electric field in causal connection with a similarly periodic magnetic field. Now what is an electric field? It is a special state, occurring even in empty space, which we can prove and measure experimentally by quite determined characteristics. . . . Thus an abstract play of mathematical symbols and numbers has replaced concrete imaginable ideas.—G. Mie, *Naturwissenschaft und Theologie*, 1932, p. 18.
>
> We can reach a satisfactory theory only if we give up the aether-hypothesis. Then the electromagnetic fields constituting light appear, no longer as states of a hypothetical medium, but as subsistent structures (*selbständige Gebilde*) which are sent out from the light-source exactly as in Newton's emission-theory. And, just as

> in this theory, a space, which is free from ponderable matter and is not penetrated by radiation, appears as really empty.—Einstein, PZ 10 (1909) 819.

These quotations refer to electromagnetic waves. By skilful emphasis on 'empty space' they seem to suggest some great advance on Maxwell's equations. One is led to imagine that the denial of something called 'a medium' is of vital importance, that it 'gradually began to win ground' as a result of new theories and fresh experiments. Above all, the picture of these waves as *propagated subsistents*, combined with the allusion to Newton's emission-theory, is calculated to lead the unwary reader to imagine that Maxwell's equations and Liénard's force-formula have been abandoned. It would even seem that the authors have almost succeeded, by dint of their own change of vocabulary, into deluding themselves into the belief that they have got rid of schesic velocities. At any rate there is no doubt that the adoption of this almost-ballistic language owes its entire plausibility to the obvious implication that only relative velocity occurs. But these authors, in their analytical work if not in their discourse, repudiate any such conclusion. Without so much as mentioning Ritz, they reproduce Maxwell's equations; i.e. they are logically bound to accept Liénard's force-formula with its schesic velocities v and v'. These inconvenient quantities are not disposed of; they are merely buried under an accumulation of verbiage.

Finally, we may ask a very ordinary question which will seem an anti-climax. Have these writers really got some secret up their sleeve? Have they some esoteric explanation for the propagation of light, not vouchsafed to a pedestrian physicist who has to discuss and compare the formulae of Liénard and Ritz? Well, here is the answer:

> How then is the relativist to answer the question: By what means is light transmitted if there is no medium for the transmission, and where or in what is the sun's radiation located during the time of transmission? He will answer simply that such a question can be asked only by one who has failed to appreciate our position in regard to our world of phenomena. In no case whatever, not even in that of the simplest phenomenon, can the question *by what means* or *why* be answered. Explanations never explain, they merely describe. . . . The very idea of explanation is beyond our understanding, just as is the idea of creation. All we can do is to take things as we find them.—F. R. Denton, *Relativity and Common Sense*, 1924, p. 12 f.

So after all, the science of physics contains nothing but quantitative description. What then are we to think of all these disputes about lines of force, aether, space, field, and medium ? These terms are not meant to explain ; they do not enter into our formulae, except in so far as they may be regarded as picturesque circumlocutions. The only scientific issue is this : Does a writer employ schesic velocities or does he use purely relative velocity ?

Apply this criterion to the following declaration :

> There is some process at the source and some [subsequent] accompanying process at the sink [receiver], and nothing else as far as we have any physical evidence ; furthermore, the elementary act is unsymmetrical, in that the source and the sink are physically differentiated from each other. This is the most complete expression of the physical facts ; there is nowhere any physical evidence for the inclusion of a third element—the ether. Therefore all the phenomena apprehended by an observer—and this embraces all physical phenomena—can be determined only by the source and sink and the relation to each other of source and sink, for there is nothing else that has physical meaning in terms of operations. This formula covers not only the possibility of such first order phenomena as aberration and the Doppler effect, but also shows that such second order effects as that looked for by Michelson and Morley must be non-existent.—Bridgman, i. 165 f.

Optics being beyond our scope, let us apply this language to the force exerted by one charge S (the 'source') on another R (the 'receiver'). There is some process at S and a subsequent process at R. If 'there is nowhere any physical evidence for the inclusion of a third element,' the schesis, then the velocities v and v' do not occur in the expression for the force. It cannot, however, be maintained that 'the relation to each other' of S and R, i.e. their relative velocity u, alone 'has physical meaning in terms of operations.' For the measurement of the quantities v and v', or of quantities resulting therefrom in statistical applications of the force-formula, is quite in order as an experimental 'operation.' And this measurement determines the required reference-frame for these velocities. We must therefore reject Bridgman's somewhat *a priori* argument in favour of Ritz ; for this is what the passage amounts to, though the author is quite unconscious of the implication. Like most of his contemporaries, this critical-minded American professor believes in the schesis, but objects to calling it 'the ether.'

5. Relativity.

Though the specific investigation of the special theory of relativity is beyond the scope of the present volume, it has already been found necessary to refer to Einstein's views, and further reference will be made in the next chapter. Any discussion of the aether would nowadays seem incomplete and faulty if the attitude of relativists to the problem were not at least recorded. Accordingly we propose to catalogue these views concisely under six headings. We shall do our best, in the brief space at our disposal, to reconcile the obvious discrepancies which emerge. Even this short enumeration will at least have the effect of showing that this new theory is by no means as logical and comprehensible as it is usually represented to be.

It must be emphasised that, inasmuch as we are exclusively concerned with scientific physics as verified in the laboratory, we really have no need to consider what other mythical observers —rushing through the laboratory at 30 km. per second or riding a beta-particle—might observe. As Frenkel says (i. 270),

> We have no need of altering our usual concepts of time and space so long as they refer to a definite inertial system. Only for two different inertial systems [with two different observers] does the Lorentz transformation, according to Einstein, represent a new extraordinary conjunction of the usual space-time-magnitudes.

(i) Relativists accept the Maxwell equations and the electron theory.

There is no need of substantiating this assertion by means of further quotations; it can be verified by inspecting any book on relativity. It follows that relativists must logically accept Liénard's force-formula.

(ii) Relativists maintain that the schesis—the reference-system for velocities in Liénard's formula—is the earth in its (orbital) motion, i.e. the laboratory.

Some typical quotations may be given to show that Einstein and his adherents do maintain this.

> The system of coordinates is fixed with reference to the observer. —Houston, p. 229.
>
> *E* is the force on a unit charge fixed with respect to the observer.— Swann, i. 369.
>
> An electric field . . . is conveniently specified in terms of the

electric intensity. . . . [This is the force] on a unit positive test-charge at rest relative to the observer.—Leigh Page, x. 216.

The meaning of the phrase 'at rest' is of course only conventional. For the present we may take it to mean 'at rest relative to the laboratory.'—Biggs, p. 3.

When we speak of a charged body being at rest or in motion, we mean of course at rest or in motion with regard to the earth.—Barnes, *Scientific Theory and Religion*, 1933, p. 195.

The force exerted on a unit charge is $\mathbf{E} + c^{-1}V\mathbf{v}\mathbf{H}$, where $\mathbf{v}$ is the velocity of the charge relative to the system of instruments used to measure the forces.—Richardson, p. 205.

The velocity $\mathbf{v}$ of the electricity is supposed to be measured relatively to the material system on which the observer is. In all ordinary cases $\mathbf{v}$ will be the velocity relative to the earth or to the laboratory. . . . It is customary in electrical experiments to regard the laboratory as at rest.—H. A. Wilson, ii. 3.

The practical electrician invariably thinks of the earth as being at rest in the aether.—E. Cunningham, *The Principle of Relativity*, 1914, p. 52.

All experiments have shown that electromagnetic and optical phenomena, relatively to the earth as the body of reference, are not influenced by the translational velocity of the earth.—Einstein, *The Meaning of Relativity*, 1921, p. 29.

The terminology is, of course, rather peculiar ; it seems curious to drag in 'the observer' when what is really meant is the terrestrial laboratory. But, as in ordinary life we say that hard words break no bones, so in physics we can say that idiosyncracies of terminology alter no formulae.

(iii) Unfortunately relativists also maintain that the schesis is the reference-system of the fixed stars.

The words which have been italicised in the following quotations show that, according to relativists, the basis of this position is variously claimed to be : science, electromagnetic theory, electrodynamics, the theory of electrons, all the phenomena of electromagnetism.

The electron theory assumes that the electromagnetic field is located in a stationary 'ether.' . . . The existence of an all-pervading ether is disproved by the results of three experiments.—Joos, p. 443.

Science was inevitably led to the idea of an absolutely immovable and stationary ether.—Tolman, *Relativity of Motion*, 1917, p. 16.

It was a necessary consequence of *the electron theory* that the resulting motion of the earth and the solar system relative to the aether should have an influence of the order v^2/c^2 on the course of electrodynamic phenomena.—H. Thirring in *Geiger-Scheel*, 12 (1927) 257.

[In the Trouton-Noble experiment] *the theory of electrons,* unless it be modified by some new hypothesis, would undoubtedly require the existence of such a couple.—Lorentz, vi. 11.

The electromagnetic theory failed to explain one experiment, namely the attempt to measure the relative motion of the earth through the hypothetical all-pervading ether.—*Outline of Atomic Physics*, New York, 1933, p. 133.

Lorentz's theory of the stationary ether is brilliantly confirmed in *electrodynamics*.—Planck, p. 242.

Then came H. A. Lorentz's great discovery. *All the phenomena of electromagnetism* then known could be explained on the basis of two assumptions : that the ether is firmly fixed in space—that is to say, unable to move at all—and that electricity is firmly lodged in the mobile elementary particles. To-day his discovery may be expressed as follows : Physical space and the ether are only different terms for the same thing, fields are physical conditions of space.—Einstein, *The World as I see It*, 1935, p. 178 f.

At first sight (ii) and (iii) seem to involve a contradiction. Moreover the reason alleged for (iii) is quite incorrect. The theory of electrons, even if we confine the term to Liénard's formula and ignore that of Ritz, does not *per se* say anything about the reference-frame. *That* must be decided by experiment. It is only on p. 593 of his text-book that Jeans tells us : 'We have so far made no clear distinction between the conceptions of rest in the aether and rest relative to the walls of a laboratory.' This is mathematically true, as regards the logical development of the theory. But from the physical standpoint he should have decided long before this ; many of the experiments to which he previously refers have decided the question ; and in fact Jeans himself implicitly settled it at the beginning when he discussed 'electrostatics' as referring to charges at rest *in the laboratory*.

How do relativists attempt to reconcile (ii) and (iii) ? We cannot discuss the matter in detail here, so we shall merely give one quotation and a brief comment.[23]

In spite of the fact that we have now found five equations which . . . have exactly the same form as the five fundamental equations used by Lorentz in building up the stationary ether theory, it must not be supposed that the relativity and ether theories of electromagnetism are identical. Although the older equations have

[23] Some writers merely give both views without discussion or reconciliation. In the formula $\mathbf{F} = \mathbf{E} + c^{-1}V\mathbf{v}\mathbf{H}$, according to Schott (i. 3), $\mathbf{v}$ is the 'velocity relative to the stagnant aether or—if we prefer—relative to a system of axes fixed with respect to the observer.' If we prefer !

exactly the same form as the ones which we shall henceforth use, they have a different interpretation; since our equations are true for measurements made with the help of any non-accelerated set of coordinates, while the equations of Lorentz were in the first instance supposed to be true only for measurements which were referred to a set of coordinates which were stationary with respect to the assumed luminiferous ether. . . . Already for Lorentz the ether had been reduced to the bare function of providing a stationary system of reference for the measurement of positions and velocities; and now even this function has been taken from it by the work of Einstein, which has shown that any unaccelerated system of reference is just as good as any other.—Tolman, *Relativity of Motion*, 1917, p. 173.

The position appears to be this:

(*a*) The Liénard formula, as proved say in this book, necessarily implies 'a stationary aether,' i.e. the velocities v and v' *must* be referred to the Newtonian inertial system of the fixed stars. Why is this gratuitous and extraordinary statement made? The reason appears to lie in the undeserved prestige of *the additional hypothesis* of a stationary aether which Lorentz tacked on to the electron theory; and also in the misinterpretation of certain results in optics.

(*b*) Einstein claims to effect a reconciliation by saying that (iii) is true for an observer at rest with respect to the fixed stars (and consequently hurtling rather rapidly through the laboratory), while (ii) is true for the scientific observer in the laboratory. That is, he agrees with Lorentz for the mythical stellar observer, and he agrees with the contention made in the present book (schesis, if it exists, = laboratory) for *scientific* physics. It is only with this latter that we profess to be concerned; we are in agreement with relativists on *this* point. As to all the extra-scientific fantasies we remain cheerfully and aggressively indifferent.

The concession of an earth-convected aether is sometimes couched in peculiar terms:

The principal conclusion that follows from the relativity theory is that the motion of the earth through space makes no difference, so that it is perfectly proper to regard the earth as at rest. The average man has been in the habit of regarding the earth as at rest for several thousand years, and so now has the satisfaction of knowing that he has been conducting his affairs in strict accordance wlth Einstein's epoch-making discoveries.—H. A. Wilson, *The Mysteries of the Atom*, 1934, p. 83.

It is difficult for our physics-popularisers to avoid paradoxes and witticisms even when stating the commonplace. So Einstein's epoch-making discoveries are cited as justifying Ptolemaic astronomy and neolithic prejudice! But the issue is not whether the earth is at rest (whatever that means), but whether the schesis is at rest relatively to the earth. Hence when the same writer tells us (ii. 3) that 'it is customary in electrical experiments to regard the laboratory at rest,' he means that electricians regard the schesis as at rest in the laboratory; and he now adds that relativists do likewise. A more accurate statement would be that in electromagnetic experiments the only velocities occurring are either (*a*) velocities of charges relative to the laboratory or (*b*) the relative velocities of moving charges *inter se*; this leaves the issue open between Liénard and Ritz.

(iv) Some relativists hold that the velocities (v and v') occurring in Liénard's formula cannot be determined at all.

This peculiar position seems to be upheld in the following passages.

> The principle of relativity is the general hypothesis, suggested by experience, that, whatever be the nature of the aethereal medium, we are unable by any conceivable experiment to obtain an estimate of the velocities of bodies relative to it.—Cunningham, p. 155.
>
> The principle of relativity demands the renunciation of the assumption . . . of a substantial carrier of electromagnetic waves. For when such a carrier is present, one must assume a definite velocity of a ponderable body as definable with respect to it; and this is exactly what is excluded by the relativity principle. Thus the ether drops out of the theory.—Planck, *Eight Lectures on Theoretical Physics*, 1915, p. 118.
>
> We say 'Let v be the velocity of a body through the aether,' and form the various electromagnetic equations in which v is scattered liberally. Then we insert the observed values and try to eliminate everything that is unknown except v. The solution goes on famously; but just as we have got rid of the other unknowns, behold! v disappears as well and we are left with the indisputable but irritating conclusion: $0=0$. . . . 'Velocity through aether' is as meaningless as 'north-east from the north pole.'—Eddington, *Nature of the Physical World*, 1928, p. 30.
>
> There is a fundamental property by virtue of which an electric field differs from a mechanical substance. A mechanical substance has a definite state of motion. . . . For the electric field, however, no corresponding certainty is possible. We can imagine two observers on different vehicles, who cut through the electric field with different but relatively uniform velocities; neither of the two

would be able to say that he alone was at rest relative to the electric field.—H. Reichenbach, *Atom and Cosmos*, 1932, p. 134.

As regards Planck's denial of a 'substantial carrier,' that is his personal philosophical opinion which has nothing to do with physics. Reichenbach's imagination about two observers on different vehicles is also his own private affair. As to what Eddington means, the present writer must confess that he cannot make head or tail of it. Certainly in the present book, it has not been our experience that when we said 'let v be the velocity of the body or the electron through the aether' we found ourselves left with : $0 = 0$. These quotations seem definitely to say that we cannot determine v and v' in Liénard's formula. This seems utterly irreconcilable with the other contentions of these writers, e.g. (i) ; and if it were true, it would mean the adoption of Ritz's formula. If our interpretation is wrong, what *do* these passages mean ?

(v) Relativity has abolished the aether or at least made it an unnecessary and useless hypothesis.

We have already abundantly illustrated this contention by quotations from relativist writers. We have tried to show that the assertion is merely a dispute about the *word* 'aether' and is of no scientific consequence.

(vi) According to 'relativity' all motion is relative ; hence only relative velocity can occur in the formulae of physics.

This thesis is enunciated in the following typical quotations.

Nature is concerned only with relative velocities.—Jeans, *New Background of Science*, 1933, p. 94.

Relativity is the theory of relative motion. . . . All motion must be considered as relative.—*Outline of Atomic Physics*, 1933, p. 253.

Since our experience is confined to relative motions, it ought to be possible to express the laws of motion in terms of relative motions alone without any reference to absolute motions. This in effect is what Einstein has done.—G. B. Jeffery, *Relativity for Physics Students*, 1924, p. 19.

The rate of change of position of objects relative to one another . . . is the only observable quantity of this type which there is any meaning in using in our description of the physical world.—F. A. Lindemann, *Philosophy*, 8 (1933) 21.

All the effects are made to depend on the relative motion of matter. It is in fact quite unnecessary ever to bring the word 'aether' into the discussion.—Richardson, p. 323.

That branch of electrical science which deals with the properties of electrical charges when at rest is called electrostatics. . . . By

> charges 'at rest' we mean at rest relatively to one another. We shall see that there is no evidence for the view that the absolute motion of the charges affects their action on one another.—Richardson, p. 12.

The last quotation, the only one which draws an immediate scientific conclusion, is a clear statement of Ritz's position. But it is entirely illogical and out of place in a Maxwellian text-book which is pledged to Liénard's formula. If the etymological connection of 'relativity' with 'relative motion' can thus deceive even the expounders of the theory, we naturally begin to suspect the existence of widespread confusion concerning elementary distinctions.

To get rid of purely verbal discussion, let us avoid words like 'absolute' and 'aether.' In Liénard's formula, to which every relativist implicitly or explicitly adheres, the schesic velocities v and v' occur; in Ritz's formula only the relative velocity u of the two point-charges occurs. And of course the same distinction reappears in the respective statistical conclusions drawn from the two rival laws. This distinction is scientific, it is directly reflected in the formulae, it is amenable to experimental test. In the one case we have one quantity, the relative velocity of A and B; in the other case we have two quantities occurring unsymmetrically: the velocity of A relative to C and the velocity of B relative to C. Now some relativists admit that they have no objection to calling these latter velocities 'absolute.'

> It is self-evident that we cannot speak of the absolute rest of the aether; the expression would have no meaning.—Lorentz, i. 4.
>
> Clausius's fundamental law, which is derived without any reference to a medium, cannot do without the co-operation of such a medium, for a really 'absolute' velocity is not physically definable. —M. Planck, *Das Prinzip der Erhaltung der Energie*, 1913[3], p. 272.
>
> Bodies at rest with respect to this system of axes fixed in the ether would be spoken of as 'absolutely' at rest, and bodies in motion through the ether would be said to have 'absolute' motion.—Tolman, *Relativity of Motion*, 1917, p. 17.
>
> In physics we should not be quite so scrupulous as to the use of the word 'absolute.' Motion with respect to aether or to any universally significant frame would be called absolute.—Eddington, *Nature of the Physical World*, 1928, p. 30.

We have indeed so used the word 'absolute' several times in this book. But it has been found advisable to abandon it in the present discussion; the word has misleading associations and

relativists use it in several senses. So we call the velocities v and v' *schesic* in contradistinction to u the *relative* velocity. Now we admit as experimentally proved that the schesis or reference-frame is the laboratory. But it is surely a quibble to infer from this our right to call all velocities indiscriminately 'relative,' thereby ignoring a clear distinction which should be reflected in our terminology. Of course, these schesic velocities must be relative to *something*. If the aether were 'stationary' we could take the something to be the fixed stars; we could in any case invent the body Alpha to embody our axes!

Hence we can apply this distinction to the 'relativist' thesis that all motion is 'relative.'

(*a*) The obvious meaning of this proposition—the meaning which many relativists delude themselves into implying—is that laws can involve only the relative velocity of the entities primarily concerned without any reference to a *tertium quid*. For example, the force between two electrons A and B can thus involve only the relative velocity of A and B. But this is decidedly *not* what is meant. Relativists are not quite so *relativist*! They draw the line at being as radical as Ritz.

(*b*) The thesis must therefore be reduced to the innocuous statement that all velocities are relative to something and that this something, this frame or schesis, can always be identified with some material configuration such as the fixed stars or the laboratory. The only comment necessary is that no one ever denied this proposition, hence its emphatic proclamation as if it were a revolutionary slogan is a ludicrous anti-climax. Relativists are not rushing in where angels fear to tread; they are marching stolidly along the broad highway of common sense.

In the light of this distinction we can perhaps find the idea which inspired the passages quoted under (iv), though we cannot justify their language.

> [Einstein's] principle of relativity [is] that the laws of nature are such that no experiment can reveal an absolute velocity or—what comes to the same thing—a velocity relative to the ether.—Jeffery, *Relativity for Physics Students*, 1924, p. 55.

No one would deny that medium-velocities occur. For example, the Doppler effect in sound depends on $(1 - u/c)/(1 - v/c)$, and not simply on the relative velocity $(u - v)$ of the source and receiver. Now if this statement of Einstein's 'principle' meant

the repudiation of analogous formulae in electromagnetics, if it amounted to a denial of schesic velocities, it would be interesting and important, it might even be fairly termed revolutionary. But this is Ritz's principle, not Einstein's. What then can be the meaning of the assertion that no experiment can reveal a velocity relative to the ether ? It cannot mean that experiment is unable to measure the v and v' of Liénard's formula which all relativists accept. It must be merely an emphatic and peculiar way for announcing Stokes's old theory of a convected aether or, as we put it, schesis = laboratory. Convinced strongly of this, relativists then proceed to deny, one might almost say indignantly, that any electromagnetic or optical experiment performed in the laboratory involves measuring a velocity relative to the fixed stars. This appears to be the only ascertainable scientific meaning of proposition (vi).

Relativists, however, have a further object in asserting that all velocity is 'relative': they wish to emphasise that it is only the velocity of light which is 'absolute.'

> The absolute quantity s is a combination of distance and time. Its distance and time components are different for different observers, but its value is the same for them all. . . . Space and time are combined into a single absolute quantity s equal to $(r^2 - c^2t^2)^{\frac{1}{2}}$. The separation of s into two parts, r and $ct\sqrt{-1}$, is a purely relative operation of no real significance. . . . The velocity of light is the same for all observers, so it satisfies this necessary condition for an absolute velocity.—H. A. Wilson, *The Mysteries of the Atom*, 1934, p. 90 f*.

We shall in the next chapter deal briefly with the quantity called 'interval.' We are here concerned only with this entirely novel use of the word 'absolute.' We must make a distinction.

(1) For any one observer—and in fact for the only observer relevant to science, the man in the laboratory—there is a distinction between the relative velocity of A and B and the two separate velocities of A and B (i.e. relative to C). If we call $\mathbf{u} = \mathbf{v} - \mathbf{v}'$ *relative* velocity, we must apply some other epithet to $\mathbf{v}$ and $\mathbf{v}'$ separately. They are often called *absolute* velocities; but relativists have misappropriated this adjective. They should therefore welcome my suggestion of *schesic*. And, as they unanimously accept Liénard's formula, they must admit the necessary framework which, to avoid hurting their feelings, I term the *schesis*.

(2) Having settled this point, I really have no further interest in questions of lexicography. But it may tend to clarity to remark that relativists reserve the epithet *absolute* for those measure-numbers which—they allege—have the same value for two mutually moving observers. It does not fall within the scope of a book on experimental science to discuss whether such quantities exist as a matter of verifiable fact; for science has no cognisance of these moving observers. So we merely record the fact that the word 'absolute' has nowadays come to possess an esoteric meaning for the initiated. But it has no connection with the problem of the aether (*alias* schesis) which we have been discussing.

CHAPTER XIV

THE SYMBOLS OF PHYSICS

1. Basic Measures.

We have already given a simple precise account of electric and magnetic units, from which the usual complicated 'tables of dimensions' were conspicuously absent. So far as all relevant theoretical or practical problems are concerned, our treatment was complete, and we might be content to let it stand. Unfortunately we must now proceed, mostly by way of negative criticism, to justify our elementary intelligible account of a subject which has not only become a bugbear to students but has misled international congresses of experts into talking nonsense. As long ago as 1892 Duhem (iii. 458) could write:

> An exposition of the principles which govern the choice of electrical units might seem somewhat out of place in the present work, inasmuch as it is of an elementary character and can be found in numerous text-books. Thus our first intention was not to stop to examine these principles. But an attentive perusal of the treatises and text-books used in teaching revealed to us how much these apparently simple principles were generally misunderstood. There are serious errors in the pages which several writers devote to electrical units.

The same is true to-day; in fact the position is much worse owing to the recklessness of the assertions made by relativist writers. As Bouasse (i. 420) says, 'the collection of stupidities formulated in connection with dimensions truly exceeds the limit of what is reasonably allowable.' The issue, though it vitally affects the treatment of electricity and magnetism, in reality concerns the most elementary and fundamental notions of all quantitative science. It is amazing to discover that such uncertainty should prevail concerning the very meaning of the symbols used in physics. In 1922 Prof. Bridgman of Harvard

published a book on *Dimensional Analysis*. Reviewing a reprint of it, Dr. Norman Campbell says [1]:

> The whole position is not creditable to science. The differences that divide Prof. Bridgman from his critics are not matters of opinion; they concern the validity of certain quite simple logical arguments; one side in the dispute must be definitely right and the other definitely wrong. Let us hope that the appearance of this reprint will encourage impartial examination of the controversy and lead to the final establishment of the truth.

In 1933 Prof. A. W. Porter published a book on *The Method of Dimensions*. Prof. Bridgman,[2] reviewing it, declares that 'the few critical comments that the author does venture to make seem to me to reveal a quite inadequate grasp of the fundamentals of the whole subject.' It is obvious therefore that we must go back to elementary first principles before we can hope to dissipate the confusions and misunderstandings which prevail on the subject of units and dimensions in electromagnetics.

At the commencement of a book on proportion,[3] a subject fundamental to both geometry and physics, we are told: 'No attempt will be made to give a general definition of the term "magnitude." It is sufficient to give a number of examples, e.g. lengths, areas, volumes, hours, minutes, seconds, weights, etc., are called magnitudes.' All we can say is that magnitudes which are conspecific, or of the same kind, are such that one is greater than, less than or equal to another; thus one spatial length may be greater than another, a duration may be less than another duration. For convenience we need symbols to *designate* magnitudes, as a short way of referring to them.[4] For this purpose we shall use capital letters. Thus L stands for a Length, a particular instance of spatial magnitude, which like all other *qualia* is

[1] *Nature*, 130 (1932) 8. 'These references will impress the reader with the difficulties of and divergences of opinion upon the subject of units.'—Hague, p. 18 note. 'Dimensional Theory, as concerns electrical entities, is still a subject of debate.'—Lanchester, p. xv. 'Scientific workers have been squabbling for the last fifty years about electrical dimensions.'—Howe, ii. 45.

[2] *Rev. Sci. Instruments*, 4 (1933) 631.

[3] M. Hill, *Theory of Proportion*, 1914, p. 1. *Cf.* the comment of an old scholiast on Euclid's Fifth Book: 'The object of the Fifth Book is to treat of proportions; the book is common to geometry, arithmetic, music, and in general to every mathematical science.'—Heiberg, *Euclidis Opera*, 5 (1888) 280.

[4] 'It is fundamentally impossible to define left-handed screw in language alone, but all that we can do is to point to one as an example.'—Bridgman, iv. 20. Exactly the same statement applies to every spatial property.

ineffable; we can intuite it, we can point it out, but we cannot define or describe *spaciness* in terms of something else. We can, however, compare one instance L with another L'; the ratio $l = L'/L$ is called the measure of L' in terms of L as unit. This ratio of two Lengths we shall call the *length*, i.e. the number l is the length of L' when L is taken as unit.

Our usage [5] is identical with that of De Morgan:

> A capital letter denotes a magnitude, not a numerical representation but the magnitude itself; while a small letter denotes a number. . . . Let A represent a magnitude—not, as in algebra, the number of units which it contains, but the magnitude itself; so that if it be, for instance, weight of which we are speaking, A is not a number of pounds but the weight itself.—*The Connexion of Number and Magnitude*, 1836, pp. iv, 3.

The introduction of capital letters as designative symbols for units is due to Gauss.[6] in 1833. In 1863 Maxwell and Jenkin (p. 61) introduced the square bracket, which has since become so popular and so fruitful of misunderstanding:

> The name of every quantity consists of two factors or components and may be written thus $Q[Q]$. The first or numerical factor Q is a number, integral or fractional. The second or denominational factor $[Q]$ is the name of an individual thing of the same kind as the quantity to be expressed, the magnitude of which is agreed on among men. . . . We shall use the symbols $[L]$, $[M]$ and $[T]$ enclosed in square brackets to denote the standards or units of length, mass and time; and symbols without brackets, such as L, M, T, to denote the number of such units in the quantity to be expressed. Thus if $[L]$ denotes a centimetre and L the number 978, $L[L]$ denotes 978 centimetres.

If we consistently use small letters to denote measure-numbers we may avoid the brackets altogether. We shall therefore use

[5] When we come to section (3) we shall continue to use A, B, C to designate magnitudes; but L, M, T will be employed to denote numbers (measure-ratios). In this and in the following section we conform to current usage (in order to refute its presuppositions), i.e. we use L, M, T to denote magnitudes, chiefly those chosen as units.

[6] Gauss, *Werke*, 5 (1867) 116. Similarly Weber, xi. 542. Neither of these introduced the capital letters into their physical equations, as did Maxwell. *Cf.* Gray, iv. 326: ' That the quantity itself, and not merely its numerical expression in terms of some unit, was meant, Prof. [James] Thomson would indicate by the adjective *intrinsic*, as in " an intrinsic length," " an intrinsic energy." ' H. Levy, *Proc. Aristotelian Soc.*, 1937, p. 92: ' Take extension. I am trying to avoid the use of the word *length* for the moment, because otherwise we might confuse the *quality*—sometimes called the fact of extension—with its measurement,

qQ instead of $Q[Q]$, thus emphasising the entirely different nature of the symbols. Expressions of the form lL—for example 12·5 Metres, in which the singular 'Metre' would be more correct—are commonly but rather absurdly called concrete, qualified or denominate numbers. They are not numbers at all, but magnitudes; lL is the Length L'.

All this is of course very elementary; it is none the less necessary to be clear about it. We have spoken of a 'magnitude' and we have illustrated it by a straight line which we called a Length (notice the capital letter), i.e. a certain spatial property of a body. The letter L or L' merely designates it or points it out, it is a symbol standing for this Length, it is not a number.

The operations of arithmetic or algebra are not applicable to these symbols; the operation of *ratiofication* is of course valid and may by analogy be denoted by the same symbol (the solidus /) as is used for the *division* of numbers. We have many different measures of L', such as $l_1 = L'/L_1$ and $l_2 = L'/L_2$, according to the Length selected as unit. Any one of these may be called the length of L', so that 'length' means Length-measure and is obviously a 'mere' or 'pure' number.

The object of these simple remarks is to initiate the refutation of a widely accepted view, which is thus expressed in an authoritative German publication [7]:

> The formal signs of physical equations are as a rule to denote physical magnitude, i.e. qualified numbers. More conveniently we can regard them as symbolic 'products' of the numerical values (measure-numbers) and the respective units according to the equation: physical magnitude = numerical value × unit.

No one has done more than Maxwell to propagate this view. Thus he expresses Coulomb's law in the following form (i. 46):

$$F[F] = ee'r^{-2}[Q^2]\,[L^{-2}].$$

Or, in our notation,

$$\begin{aligned} fF &= qQ\,.\,q'Q/rL\,.\,rL \\ &= qq'/r^2\,.\,Q^2/L^2. \end{aligned} \tag{14.1}$$

Whence he deduces $F = Q^2/L^2$ or $Q = LF^{\frac{1}{2}}$ as the 'dimensions of the electrostatic unit of quantity.' Having removed these peculiar combinations of designative symbols, there emerges the

[7] *Verhandlungen des Ausschusses für Einheiten und Formelgrössen in den Jahren* 1907 *bis* 1927, ed. J. Wallot, Berlin, 1928, p. 43.

ordinary equation $f = qq'/r^2$, which in fact Maxwell gives (i. 74) as the 'law of force between charged bodies' and which he *exclusively* uses in his further treatment. We propose to show briefly that this juggling with units is absurd and that physical science, apart from laboratory measurement, is altogether concerned with pure numbers as exemplified in the equation $f = qq'/r^2$.

Now numbers such as the length l are in physics called 'physical quantities.' The term is not a very happy one and has led innumerable writers into serious errors; a change of nomenclature would be eminently desirable if it were possible to upset current usage. But the only feasible plan is to accept a vocabulary which cannot be ousted, while refusing to be led astray by false implications. Most of the difficulties concerning units and dimensions will at once disappear if we bear in mind that a physical quantity—represented by such letters as l, m, t—is a measure, a ratio, a number, an ordinary algebraic 'quantity.'[8] Theoretical physics expresses itself in some such algebraic equation as $l = gt^2/2$. The man in the laboratory has to *find* the correlated numbers l and t, length and time. He does this by *measuring*, by comparing a Length with another Length and a Time with a unit of Time; he deals with what we have called magnitudes, quantified objective entities which, when conspecific, can be compared *inter se*. The object of these practical operations is to deduce measures, i.e. to express the result in some equation such as $l = \text{constant} \times t^2$. The quantities occurring in all such equations are algebraic numbers. The best justification of this simple proposition is the fact that it will enable us to clear up the mess which has hitherto clogged the treatment of electric and magnetic units.

Hitherto we have taken Length to illustrate magnitude. We

[8] 'Every measure of a physical quantity such as mass or length is a ratio. . . . A pure number on the other hand is unaffected by any possible change of units.'—A. Ferguson, *School Science Review*, 18 (1937) 350. This position is an illogical compromise between the prevalent view and that held in the text. What is impure or non-numerical about a ratio? The ratio of two Lengths—say 2·5—is absolutely comparable with the ratio of two Times, and may be equal to it. In Euclidean language: as L_1 is to L_2 so is T_1 to T_2. The fact that one of our measure-ratios, or a particular combination of them, may happen to be *tautometric*, is altogether extrinsic and additional. There is no intrinsic or mathematical difference between the ratios q_1, q_2, q_3 and compound ratio $q_1^x\, q_2^y\, q_3^z$; but the latter may, for algebraic reasons dependent on our definitions, happen to be tautometric—this word will be presently defined.

must now briefly consider Time or Duration.[9] In the case of short time-intervals we have a primitive perception of equality and inequality just as we have for short space-intervals. This is especially noticeable in the case of auditory rhythm, e.g. the sound of a clock ticking or of an engine running or of a musical instrument playing. The first makers of water-clocks relied on them probably because the drip of the water from the funnel could be heard to be regular. Galileo[10] proceeded as follows:

> For the measurement of time we employed a large vessel of water placed in an elevated position; to the bottom of this vessel was soldered a pipe of small diameter giving a thin jet of water, which we collected in a small glass during the time of each descent. . . . The water thus collected was weighed after each descent on a very accurate balance.

Nowadays we measure long intervals by the periodic motions of the heavenly bodies (rotation of the earth), and short intervals by recurrent isochronous devices which we may call clocks. Here, as in other cases such as that of temperature, we make successive refinements of measurement by introducing corrections outside the previous limits of accuracy. For example, we can make an immediate judgment that the ordinary pendulum is isochronous, owing to the observed regularity of its ticking. With such a measure of time we can verify the laws of motion within the limits of experimental error. With more refined instruments we discover small residual errors below the limits of the initial direct observation. We can account for this by assuming (1) supplementary hypotheses (disturbing forces), or (2) a slight modification in the supposed law, or (3) a small discrepancy in the isochronism. In making a choice there is undoubtedly a mixture of convention with observation. But usually the probability is overwhelming in favour of (3), so that the law keeps its original simple form and yet accounts for apparent discrepancies without additional hypotheses. Our belief is confirmed if our improved clocks show the laws to hold with a much higher degree of accuracy than that with which we started. There are in physics many examples of this progressive intensification of accuracy by successive corrections of a system of measurement.

[9] *Cf.* A. Ritchie, *Scientific Method*, 1923, pp. 139–147; Broad, *Perception, Physics and Reality*, 1914, pp. 316–322.

[10] Galileo, *Dialogues concerning Two New Sciences*, Eng. trans., 1914, p. 179.

It will be observed that the accurate measurement of Time is surrogative or extrinsic, i.e. Time is measured spatially by the angle swept out by a hand on a dial or by the hour-angle of the sun or of a star, etc. But it is rather a monstrous perversion of this obvious fact to declare with Mach [11] that 'we can eliminate time from every law of nature by putting in its place a phenomenon dependent on the earth's angle of rotation.' For the position-variable of this phenomenon *is* the time, just as the length of the mercury-column (appropriately graduated) is the temperature-measure. But neither time nor temperature is *defined* to be angle or length. The thermometer replaces the rough and limited indications of our senses founded on direct perceptions. We then attribute to it a greater accuracy and use it to correct our perceptions.

Rough observation shows that the mercury thermometer has two properties: its level depends only on the temperature and rises with it. We conclude that melting ice is at an invariable temperature. Once this law is obtained, we regard it as more certain than the property of the thermometer which led to its discovery. Hence we can logically *correct* the thermometer for deformation of the bulb, we can speak of the displacement of the melting-point, and so on. In exactly the same way, as already outlined, we can correct our time-measure by successive refinements, all based on the idea of Duration as *sui generis* and on the ideal of *uniform* rotation as progressively attainable. The question of the variability of the sidereal day would be nonsense if we had *defined* earth's rotation as uniform, i.e. if absolute time were identical with sidereal time.[12]

We must therefore reject such statements as the following:

> There can be no measurement of time without a knowledge of the laws of nature. . . . So we must conclude that, at least in physics, there is not and cannot be a *true* time.—G. Jaffé, *Zwei Dialoge über Raum und Zeit*, 1931, pp. 64, 66.
>
> Nobody has ever measured a pure space-distance nor a pure

[11] *History and Root of the Principle of the Conservation of Energy*, Chicago, 1911, p. 60. So Lindsay and Margenau, *Foundations of Physics*, 1936, p. 74: 'From the point of view taken here, the use of the time-parameter in physics may be looked upon as a matter of convenience and nothing more. All measurements reduce in the last analysis to *space* measurements.'

[12] *Cf.* Sir F. W. Dyson and R. T. Cullen, 'Variability of the Earth's Rotation,' *Monthly Notices of the R.A.S.* 89 (1929) 549. Many such articles occur in astronomical literature.

lapse of time. . . . We can even go further and say that time cannot be measured at all. We profess to measure it by clocks. But a clock really measures space, and we derive the time from its space-measures by a fixed rule. This rule depends on the laws of motion of the mechanism of the clock. Thus finally time is defined by these laws. . . . About the reality of time, if it has any, we know nothing.—W. de Sitter in Bird, *Relativity and Gravitation*, 1921, p. 209.

Newton's First Law says that every body remains at rest or in uniform motion in a straight line, unless acted on by a force. If this is examined critically, it is mostly a matter of definition. For example, How are we to know that it is not being acted on by a force? Because it is moving uniformly. Or again, how can we verify that its speed is uniform? By a clock. How do we know that a clock keeps time? By the laws of dynamics. How do we know the laws of dynamics? Why, from Newton's First Law.—C. G. Darwin, *The New Conceptions of Matter*, 1931, p. 53.

Nowadays the measure of time is based strictly speaking on a vicious circle. . . . Poincaré showed quite clearly that the measure of time has been chosen so that the equations of mechanics should be true.—G. Juvet, *La structure des nouvelles théories physiques*, 1933, p. 10.

Such utterances are symptomatic of the subjectivistic tendencies introduced by relativists into modern physics. They are supported by no serious argument, they are designed merely to make it easier to swallow Einstein's peculiar theory; to effect this the writers are ready to turn the whole science of mechanics into a gigantic fraud and to belie our fundamental perceptions. We may summarise the counter-arguments already given by the following quotation from Prof. Broad:

It seems quite clear that the *meaning* of uniformity or of isochronism has nothing to do with the laws of motion. People judged certain processes—such as the swings of pendula, the burning of candles in the absence of draughts, the descent of sand in hour-glasses, etc.—as isochronous long before they had thought of the question whether forces were present or absent. . . . This implies that under favourable circumstances we can directly judge equality of time-lapses just as we can judge equality of lengths.—*Scientific Thought*, 1923, p. 158.

There is, of course, a certain amount of convention in all modern refined methods of measurement which go far beyond perceptual observation. Without such practical judgements, based on a tremendous balance of probability, there would be no physics. The reckless statements now currently made about

time-measure [13] would be equally applicable to other parts of physics; we could no longer speak of the invariability of the metre-standard or correct a thermometer, if this new conventionalism were accepted. So dogmatically is this far-fetched attitude now accepted that Sir Arthur Eddington could write:

> I have no notion of time except as the result of measurement with some kind of clock. Our immediate perception of the flight of time is presumably associated with molecular processes in the brain which play the part of a material clock.—*Space Time and Gravitation*, 1920, p. 13.

Which is surely putting the cart before the horse! Only a very up-to-date person, habituated to a wrist-watch, would affirm that he has no notion of time without a clock; or that there is a clock in our brain which, though we cannot read it, tells us the time. It is not a healthy position for physical science to become so divorced from reality and to confuse means with the end. In view of the foregoing it is not surprising to find the same writer telling us:

> The great thing about time is that it goes on. . . . Something must be added to the geometrical conceptions comprised in Minkowski's world before it becomes a complete picture of the world as we know it. . . . Without any mystic appeal to consciousness, it is possible to find a direction of time on the four-dimensional map by a study of organisation. . . . The practical measure of the random element which can increase in the universe but can never decrease is called entropy.—Eddington, *Nature of the Physical World*, 1928, pp. 68, 74.

Prof. Bridgman [14] rightly ridicules Eddington's view of 'time's arrow,' which has been 'hailed in so many quarters as being of such unique profundity.' This is his criticism:

[13] 'The basis of our time-system is the *postulate* that the earth rotates uniformly on its axis. There is no meaning in asking whether it *really* does so unless we adopt some more fundamental standard of time-reckoning which must be equally arbitrary.'—H. Dingle, *Science and Human Experience*, 1931, p. 56. 'There is no absolute time. When we say that two periods are equal, the statement has no meaning and can only acquire a meaning by convention.'—Poincaré, *Science and Hypothesis*, Eng. tr. 1905, p. 90.

[14] *Science*, 75 (1932) 423. *Cf.* H. Spencer Jones, *Science Progress*, 30 (1936) 533: "This seems to me to be arguing in a circle; that the entropy of an isolated system increases with the time is a law that is based on experience; numerous experiments have shown that the entropy is always greater at the later instant. But how do we determine the later instant? Surely by our consciousness.'

In no case is there any question of time flowing backward, and in fact the concept of a backward flow of time seems absolutely meaningless. For how would one go to work in any concrete case to decide whether time were flowing forward or backward ? If it were found that the entropy of the universe were decreasing, would one say that time was flowing backward or would one say it was a law of nature that entropy decreases with time ? It seems to me that in any operational view of the meaning of natural concepts, the notion of time must be used as a primitive concept, which cannot be analysed and which can only be accepted, so that it is meaningless to speak of a reversal of the direction of time. I see no way of formulating the underlying operations without assuming as understood the notion of earlier or later in time.

There are several errors of confusion committed in the views generally propounded by popularising physicists.

(1) As already shown in Chapter IX, we must distinguish between the two measures : date and duration. The date measured from an arbitrary origin and reaching to $\mp\infty$ is merely a mathematical device, which enables us to express duration-measure as the difference of two dates. It is only the durations of the relevant processes which occur in physical laws.

(2) We must distinguish between real Duration and the measure-number duration. It is rather naïve for a physicist to be astonished that the number 20 is so unlike 'the time of experience'—it is also quite unlike the Length, Force and Sound of experience. But why should we expect such nonsense to occur in physics ? Common sense has no objection to offer against what alone is asserted, namely, that two quantised entities are in the ratio 20 : 1.

In connection with the scientific measurement of Time, there is a widespread fallacy, chiefly due to Bergson, which may be illustrated by some quotations :

If we observe that science operates exclusively with measures, we shall see that as regards time science counts instants and notes simultaneities but has nothing to do with what happens during the intervals. . . . We cannot measure it without converting it into space.—Bergson, *Creative Evolution*, 1912, pp. 76, 83.

The permanently valuable feature of his [Bergson's] treatment of succession appears to me to be simply his insistence on the real and profound difference between *durée réelle* and the artificial 'mathematical' or 'clock' time of our scientific manuals.—A. E. Taylor, *The Faith of a Moralist*, 2 (1930) 338.

The time of the mathematician is a one-dimensional continuum, reaching forward and backward to plus and minus infinity, every-

where homogeneous, and with an origin which may be situated arbitrarily. . . . What could be more unlike the time of experience ? —Bridgman, iv. 29.

(3) Why should we confuse the whole qualitative content of experience with its temporal aspect ? Nobody interprets Duration as a separable substance ; all we say is that certain phenomena are durational. So-and-so is happening *while* the hour-hand is moving from 10 to 11. This is a simple fact of experience ; it does not imply any reduction to clock-time, especially if this last is taken to mean a series of abstract numbers.

We have now vindicated the idea of Duration as a magnitude 'which cannot be analysed and which can only be accepted,' exactly as is the case with Length. The arbitrarily chosen units of these magnitudes we have designated L and T. We now come to a third alleged magnitude, M the unit of Mass. And here we are on much more doubtful ground, for we have no direct intuitive perception of any such category of magnitude. If we start with mass as more fundamental than force, we must begin with the equation of conservation of momentum :

$$m(\mathbf{v}-\mathbf{u}) = -m'(\mathbf{v}'-\mathbf{u}')$$

which can be roughly verified by means of trolleys or a vector balance. It is found experimentally, by using different bodies, that

$$\frac{m_1}{m_2} = \frac{m_1}{m_0}\Big/\frac{m_2}{m_0},$$

so that 'mass' is definitely associated with each body as its 'property.' But the equation from which we started contains only numbers ; we nowhere meet with a magnitude to be called Mass. On the other hand, Force appears to be a special category of experience. 'Muscular force,' says Dr. N. Campbell (iv. 71), 'is something appreciated by direct sensation ; when we set a heavy body in motion by the action of our limbs, we experience certain sensations which everybody knows and nobody can describe.' We can even estimate weights with a certain amount of accuracy. By means of well-known analogies and experiments we can pass from muscular to static force ; we can represent a spring as exerting force, a measurable property of the spring in any particular state of stretch. We next proceed to dynamic force, thanks to Galileo's experiments on the motion of a body when the balance of static forces on it is upset. So long as we

are dealing with statics, we can take Force as a magnitude which we extrapolate into other bodies from our own experience. We can then measure it in the usual way, by initially assuming Hooke's law for springs, i.e. by a surrogative or extrinsic process dependent on spatial measurement; or we can use a balance which, being a rigid equal-armed lever, also presupposes length-measure. But in dynamics we take force $=$ mass $\times$ acceleration, i.e. the product of two numbers. It seems best therefore to prescind altogether from the extremely doubtful category of Mass and to treat physics independently of the magnitude Force. We can regard force and mass as numbers which emerge in our experiments after we have measured space and time, without denying that there is some objective entity corresponding to these numbers.[15]

Whatever view may be taken of this last point, it is certain that we can build the whole of mechanics on the measure-numbers l, m, t. These we propose to call *basic* measures[16]; since all other symbols occurring are definable in terms of these, and therefore are pure numbers. To illustrate the prevailing confusion we may quote a statement from a Report of the English Mathematical Association Committee on the Teaching of Elementary Mechanics:

> It should be permissible to treat elementary problems on the accelerations produced by forces by simple proportion:
>
> $$\frac{\text{force acting}}{\text{acceleration produced}} = \frac{\text{weight}}{g}$$

Inasmuch as this refers to measures ($f/a = w/g$), the equation seems harmless enough. But a distinguished professor[17] promptly pounced on it, declaring that the correct version is:

$$\frac{\text{acceleration}}{g} = \frac{\text{unbalanced force}}{\text{weight of body acted on}}$$

[15] It is amazing to find relativists claiming this elementary fact as a triumph for Einstein. 'I may initiate the criticism of physical ideas by asking, for example, what is meant by mass. The answer of the classical physicist would probably have been that the mass of a body is the quantity of matter in the body the mass of a body is its material substance. The relativistic physicist, on the other hand, would say that the mass of a body is a number which is assigned by an operation of measurement.'—V. Lenzen, *California Engineer*, 10 (1931) 19.

[16] The term *fundamental* should properly be reserved for length-measure, which is primary and direct; the measures m and t, being surrogative or extrinsic, presuppose length-measure.

[17] Prof. Worthington in *The Teaching of Elementary Mechanics*, ed. Perry, 1906, p. 53.

i.e. $a/g = f/w$. 'Is a boy,' he asks indignantly, 'is a boy really to be encouraged to write down the ratios of quantities of different physical dimensions and to regard it as a simple proportion?' This extraordinary scruple is by no means antiquated, for in 1930 the Mathematical Association issued another Report on the Teaching of Mechanics in Schools. Instead of showing the absurdity of the objection, they actually endorsed it (p. 23 *):

> The fundamental equation of the subject is not used in the form $f = ma$, where the left-hand side is a force and the right-hand side something which (as explained above) is inevitably thought of by the beginner as totally different. Instead it is obtained in the form $f/w = a/g$, which states a simple proportion and in which each side of the equation is a mere ratio.

Whether $f/a = m$ is a 'mere ratio,' whether 'the beginner' is just 'inevitably' stupid, is left quite undecided. But later on we are told (p. 41): 'It is a commonplace that the letters that occur in a formula of algebra stand for numbers. The same is true of a formula in mechanics.' However, this concession to common sense is vitiated by a reference to unexplained 'dimensions' and a remark that 'it is for the teacher to decide at what stage the change can safely occur,' i.e. the change from $f/w = a/g$ to $f/a = w/g = m$. When the best teachers are so muddled about the symbols of mechanics, it is not surprising that so much confusion should prevail concerning the symbols of electromagnetics.

2. Derived Quantities.

As one of the simplest derived or compound quantities, let us take velocity: $v = l/t$. The usual version is

$$vV = lL/tT = l/t \, . \, L/T, \tag{14.2}$$

where the capital letters designate unit magnitudes. As Prof. Kennelly says (iv. 98): 'If the unit of length in a system is L (say 1 metre) and the unit of time is T (say 1 second), then the unit of velocity . . . will be $V = L/T$.' The only possible meaning assignable to this statement is: $1 = 1/1$. How can there possibly be any meaning in Length divided by Time, if we fully comprehend what is implied in these words and in the symbols which merely designate them? One might as well try to divide the smell of a flower by the ticking of a clock! This attempt to

perform arithmetical operations on non-numerical magnitudes is quite a modern innovation, due chiefly to Maxwell. If we turn to older authors we find statements such as the following:

We cannot compare together two things of different nature such as space and time; but we can compare the ratio of the parts of the time with that of the parts of the space traversed.—d'Alembert, *Traité de dynamique*, 1743, p. vii.

Taking any force or its effect as unit, the expression of every other force is no longer anything but a ratio, a mathematical quantity which may be represented by numbers or lines. It is under this aspect that we have to consider forces in mechanics.—Lagrange, *Mécanique analytique* (1788), ed. 1811, vol. i., p. 1.

Space, time and velocity are quantities of different kinds which must be referred to different units for us to compare the numbers which represent them: l being the ratio of the space traversed to the unit of length, t that of the time taken to the unit of time, the velocity is by definition given by the equation $v = l/t$.—G. Lamé, *Cours de physique*, 1 (1836) 22.

It must always be kept in mind that v and t are abstract numbers; and that v refers to some unit of space such as a foot, an inch, a yard; and that t refers to some unit of time such as an hour, a minute, a second.—Robison, *System of Mechanical Philosophy*, 1 (1822) 100.

Let us now see what attempt is made to justify the new departure. This is Clifford's defence:

Let $[V]$ denote the unit of velocity, $[L]$ the unit of length, and $[T]$ the unit of time; then $[V] = [L]/[T]$. Here the word *per* has been replaced by the sign for *divided by*. Now it is nonsense to say that a unit of velocity is a unit of length *divided by* a unit of time in the ordinary sense of the words. But we find it convenient to give a new meaning to the words 'divided by' and to the symbol which shortly expresses them, so that they may be used to mean what is meant by the word *per* in the expression 'miles per hour.' This convenience is made manifest when we have to change from one unit to another. . . . If we give to the symbol of division this new meaning and then treat it by the rules applicable to the old meaning, we arrive at right results.—*Elements of Dynamic*, 1 (1878) 49 f.

Or, as Boltzmann (iii. 284) puts it:

Instead of the quotient of the number expressing the length by the number expressing the time, we use the expression 'quotient of a length by a time.' This implies an extension of the concept of a division; the quotient length/time must be defined anew, just as we newly define the concept of a negative or fractional power, understanding thereby a fraction or root. The advantage of this new definition consists in the fact that rules for calculation proved for the former definition can be extended to the new definition.

The only argument discoverable here is Clifford's pragmatic argument that 'we arrive at right results.' We shall presently see the irrelevance of this by getting the right results without any bad metaphysics. It is admitted that the ordinary meaning of ratio is 'nonsense'; but we are left completely ignorant of the new non-nonsensical meaning; it is just a new definition of some undefined relationship between Space and Time; translated into Latin it is *per*. The extraordinary feature of this innovation is that the equation is dissociable into the familiar $v = l/t$ and the mystic $V = L/T$. The only workable intelligible equation is the one in which the symbols denote measures; when a student has to find a velocity, he measures l and t and produces v. What has happened to V and L/T? They have been neatly removed to form a second mysterious equation $V = L/T$, which is labelled 'dimensional,' as if it were an unpleasant chemical.[18]

Against this assertion of the meaninglessness of L/T, it may be urged that unit velocity is a unique interrelation between Space and Time, that we have not exhausted its meaning merely by dividing one pure number by another. As Sir Oliver Lodge says,

> changing the units does not affect the velocity of light. Whether you say light travels 186,000 miles a second or whether you say it is so many inches an hour, makes no difference to the velocity. An algebraic symbol ought to represent the thing itself, not a mere number of units. Altering the numerical specification—which is what you do by altering units—means no difference to the thing itself.—*Monthly Notices*, 80 (1919) 107.

Without raising the question of absolute or relative motion, we can certainly say that movement is a real process, either a change in intrinsic ubication or an alteration in the spatial relationship of bodies. It is unique and indefinable like Space and Time. But for that very reason it escapes through the meshes of science. We can do nothing with the spaciness of space as such; we can only, so to speak, point to it. We can indicate what we mean by saying, Let L or L' be the Length. But unless you already have the intuition of space, you will not know what is signified; the meaning is not contained in the symbol. Knowing the meaning of what is indicated, the length of L' can de defined as the ratio $l = L'/L$. While the Length L' remains the same, its length may be any number whatever, it

[18] 'The function unit length/unit time is called by Maxwell the dimensions of the unit of velocity.'—A. F. Sundell, PM 14 (1882) 87.

depends on the choice of L. It is only l which is an 'algebraic symbol,' that is, a symbol which, just because it has a meaning beyond and apart from the mere intuitional reference to space, is capable of interrelations with other symbols—or to speak more correctly, with what they symbolise. All these symbols are operators, they stand for ratiofication, which is specifically the same operation no matter what may be the kind of magnitudes which are compared in pairs. It is precisely because our symbols do *not* 'represent' things themselves, but rather a general type of quantitative operation applicable to all kinds of magnitudes, that we obtain the equations of physics. If our symbols stood for the magnitudes themselves, we could do nothing with them, each would remain in splendid isolation.

This view must not of course be distorted into a kind of algebraicism, a shadowy 'world of science' in which some paradox-infected writers profess to believe. We admit the methodological profession that theoretical science is concerned only with the algebraic interrelations of numbers. But we must refuse to accept the further implication that the qualitative world of experience, including the causal and activist nexus, is thereby ignored and even denied. There is no justification for erecting measures into monads in an occasionalist universe. For our operational symbols are neither self-contained nor self-explanatory, though the highly mathematical physicist—who has probably forgotten what the inside of a laboratory is like—is apt to think so. The symbols of physics are essentially incomplete; their full significance implies a reference beyond their numerical values; they can be understood only in their *context* or background of laboratory and life.

Since these words were written I have come across a striking assertion of the same view in the recent work of a distinguished physicist:

> The equations always have to be accompanied by a 'text' telling what the significance of the equations is and how to use them. . . . Not only must the text describe the nature of the measurement, but it must also specify the connection between the different symbols in the equation. . . . It appears, therefore, that a complete mathematical formulation requires equations plus text, and the text may perform a variety of functions. The necessity for a text is almost always overlooked, but I think it must be recognised to be essential. . . . The text cannot tell us what it is that the correspondence is to be set up with, without going outside the system of the mathe-

matical theory and assuming an intuitive knowledge of the language of ordinary experience. . . . The text contains the unanalysables of the theory and thus involves its essential limitations. . . . The text is never explicitly stated in formulations of the theory, so that we shall have to construct it for ourselves by observing how the equations are used.—Bridgman, iv. 59 f., 72.

It follows that no physical equation is self-explanatory, for it consists merely of relations between pure numbers. Thus $x^2 + y^2 = a^2$ represents a circle, only when x and y are understood to be the measures of perpendicular Lengths. Similarly $l = vt$ might mean any number of things. When we specify that l is length and t time, it does not yet define velocity v; we must further say that t is the time taken by something—say light—to traverse the distance l. But the ideas of time *occupied*, distance *traversed* and light *moving*, are not contained in the equation at all. They constitute the context, the experiential framework in which we read the equation. If we retain the same context, i.e. if we are referring to the same physical process, but use different units of space and time, we shall obtain a different number for v. The one thing certain about 'the thing itself' is that it cannot occur in an algebraic equation; all we can say is that light takes Time to travel over Space. There is no how-muchness about it; the moment you consider how much Space and how much Time, you have started to measure. It is therefore irrelevant to object that in ordinary life, without knowing anything about the formula, we distinguish between quick and slow movements.[19] We do, but only by making an estimate which is a rough measurement; in the same time one travels over a greater distance than the other, or one takes a longer time than the other to traverse the same distance. There is a comparison between two Lengths or between two Times. Or, if we prefer, we could take quickness and slowness as qualitative attributes of motion, which we correlate with greater or smaller numbers of what we call the velocity. Physics is not concerned with the nature of things, but only with an algebraic pattern which represents, or is correlated with, their behaviour *inter se*.

Whatever way we think of velocity, we must think not merely of Space and Time in some kind of juxtaposition, we must think of something *traversing* Space *during* a Time. If we like, we can

[19] A. Meinong, *Ueber die Bedeutung des Weberschen Gesetzes: Beiträge zur Psychologie des Vergleichens und Messens*, Hamburg-Leipzig, 1896, p. 15.

call this with Meinong (p. 16), a grouping of Space and Time 'in a relation by means of which they combine into a complex (*Vorstellungsgebild*) of higher order.' But Space remains Space and Time remains Time, they do not start playing leap-frog; in fact they do nothing, but something else does something—it occupies Time in travelling over Space. This phrase has now been repeated *ad nauseam*, merely to show how impossible it is to get this qualitative uniqueness into an equation. We employ algebra only at the price of eliminating all qualitative distinctions.

Against this it may be urged that

> every physical concept has an intuitional basis; the connection with this basis may not be abandoned if the full understanding of the concept is to be preserved. Thus velocity means, not the quotient l/t, which in itself is quite meaningless, but rather a special state of a body, whose exact measurement becomes possible with the help of this quotient.—Poske, cited by Meinong, p. 15.

But it must be answered that, explicitly in physical measurements and implicitly in the estimates of ordinary life, velocity does in fact mean the ratio of the two measures l and t. In one sense everything in a quantitative physical equation is devoid of non-numerical significance. We have to know the contextual complex or universe of discourse—the preliminary understanding that l is the measure of Length, and so on—in order to determine its significance for physics. We can, if we choose, use the term Velocity to designate and summarise this non-quantitative background of experience in the present case. But it would be an ambiguous use of a word already preempted for a definite measure in science.

There might be some justification for it if our procedure consisted in taking an entity called unit Velocity and in comparing with it another magnitude of the same kind. Prof. Bridgman (i. 99) thinks we can do this:

> There is still another most interesting way of defining velocity, in which the analysis into space and time is not made at all; but velocity is directly measured by building up the given velocity by physical addition of a unit velocity selected arbitrarily. . . . We may in the first place construct a concrete standard for velocity, as for example by stretching a string between two pegs on a board with a fixed weight. If we strike the string, a disturbance travels along the string, which we can follow with the eye; and we define unit velocity as the velocity of this disturbance. An object has greater than unit velocity if it outruns the disturbance and less if

> it lags behind. We may now duplicate our standard, making another board with pegs and stretched string, and check the equality of the two disturbances by observing that the two disturbances run together. We now define two units of velocity as the velocity of anything which runs with the disturbance of the string of the second board, when the second board is made to move bodily with such a velocity that it runs with the disturbance of the first string. The process may be extended indefinitely, and any velocity measured.

Prof. Bridgman is surely under a misapprehension if he fancies that he has defined or measured velocity without 'the analysis into space and time.' All he says is that something has twice the velocity of another when it traverses twice the distance in the same time. And by adjusting the weight on the second board, we could secure that the disturbance travels its whole length while that in the first covers half. A velocity would then be doubled if twice the distance is travelled in the same time or if the same distance is traversed in half the time. In other words, we are merely using the equation $v = l/t$.

We maintain, therefore, that there is no such *magnitude* as Velocity at all, there is only a measure which is defined as l/t; when we speak of measuring velocity, we *mean* the quotient of these two other measures. The so-called unit of velocity is not a standard of comparison; it is the value of v when $l = t$, and this value is 1. (Of course it may not be unity when different measures of length and time are employed; thus a velocity which is 1 when mile and hour units are used is 88/60 in foot and second units.) Therefore, the usual definition, which tells us, for example, that 'the unit of velocity is the uniform velocity of one centimetre per second,' merely tells us that unit velocity is the number *one*! The proper definition is: velocity is the length in cms. divided by the time in secs. If this number is, say, 6, we can express this as $v = 6$ cm./sec. But the symbol or phrase 'cm./sec.' is not a number—still less some undefined quality—multiplying 6; it is merely a marginal note informing or reminding us that v was found equal to 6 when length-measure in centimetres was divided by time-measure expressed as the number of seconds.

It is rather surprising to find philosophers falling into the egregious blunder which pervades contemporary physics:

> Regarding multiplication and division . . . as real operations performed on concrete quantities, the square bracket in the above symbols stands for a concrete unit. For example, the velocity

'320 feet per 60 seconds' means 320 ft./60 sec. = 16 ft./3 sec. = 16/3 of *unit velocity*. Those mathematicians who hold that such an expression as ft./sec. is meaningless have to maintain that the mathematical equations which are used to express physical facts are concerned only with the numerical measurement of concrete quantities, whereas I hold that they are concerned with the concrete quantities themselves.—W. E. Johnson, *Logic*, 2 (1922) 185.

Surely one need not be a 'mathematician' to uphold common sense. Why is it said that we 'have to maintain' that equations are algebraic, as if the view were held with reluctance or apology? The viewpoint shamelessly advocated in this chapter is that the alternative theory leads to unmitigated nonsense. It is no argument in favour of this prevalent theory merely to reiterate that ft./sec. is unit velocity. Someone should try to tell us seriously what it *can* mean except one divided by one.

We must similarly reject such a statement as that of Born [20]: 'The unit of charge in the c.g.s. [i.e. electrostatic] system must be written cm. $\sqrt{\text{grm. cm./sec.}}$' If this conglomeration of symbols is taken literally as expressing the unit of charge, it can be interpreted only as $1 \times \sqrt{1 \times 1/1}$, that is, unity. Similarly, Pohl and Roos (p. 13) give us the equation

$$1 \sqrt{\text{dyne}} = 300 \text{ abs. volt} = 10 \text{ abs. ampere.}$$

To which we can only reply that (in the c.g.s. system) the square root of a dyne is 1. And when Haas (i. 225) says that 'the dimensions of the electrostatic unit of quantity are gm.$^{\frac{1}{2}}$ cm.$^{\frac{3}{2}}$ sec.$^{-1}$, dividing this by a second we obtain the absolute current,' the operation can mean only division of 1 by 1.

Accordingly we maintain that unit velocity is neither L/T nor cm./sec.; it is the number *one*, and it is nothing else. Velocity-measure is a 'derived quantity,' a 'pure' number defined as l/t, where the solidus denotes the arithmetical division of l by t or the ratio of the two numbers l and t. If the reader finds anything strange in this statement, the reason is that he is thinking of the genesis and context of these numbers l and t. In ordinary life and in the laboratory these aspects are important; but they are entirely irrelevant to the numbers or algebraic quantities which occur in the equations of physics.

Philosophers have got into the habit of complaining that the simple measure known as velocity is a blight destroying all motion and life. Let us hear Bergson.

[20] *Einstein's Theory of Relativity*, 1924, p. 132.

> When positive science speaks of time, what it refers to is the movement of a certain mobile T on its trajectory. This movement has been chosen by it as representative of time; and it is, by definition, uniform. . . . Of the *flux* itself of time, still less of its effect on consciousness, there is here no question; for there enter into the calculation only the points T_1, T_2, T_3, . . . taken on the flux, never the flux itself. . . . And when we say that a movement or any other change has occupied a time t, we mean by it that we have noted a number t of correspondences of this kind. We have therefore counted simultaneities; we have not concerned ourselves with the flux that goes from one to another.—Bergson, *Creative Evolution*, 1912, p. 356.
>
> Science reduces movement to something other than itself, and substitutes for real duration, the stuff movement is made of, a symbolic image derived from extension in space. Thus it measures movement by bringing it to a standstill, as it analyses life by killing it.—J. Chevalier. *Henri Bergson*, 1928, p. 83.

Who would ever think that the timing of a race or the setting up of a sand-glass would have such disastrous effects? It would seem that the exponents of this view are themselves the unconscious victims of the theory we have called *algebraicism*, which they are professing to combat. Bergson thinks that physics deals only with the highly sophisticated notion of dates, that it does not concern itself with 'flux,' i.e. duration. We have already at some length demonstrated the opposite view. His followers think that motion can be measured only by stopping it. The supposition behind all this rhetoric is that a number like l/t is a lifeless abstract thing, utterly incommensurable with the vivid reality of experienced motion. Quite so. But this view is not opposed to 'science,' it is directed against an extraneous and fallacious metaphysic of which many scientific expositors have been guilty. It is surely a decisive argument in favour of clarifying the elementary logic of mensuration, when we find philosophers building upon such distortion and misrepresentation.

Analogous, though less relevant to our subject, is the curious view held by Hegelian Marxists.

> The movement of matter underlies all the phenomena of nature. But what is movement? It is an obvious contradiction. . . . A body in motion is at a given point, and at the same time it is not there. . . . Motion is a contradiction in action; and consequently the fundamental laws of formal logic cannot be applied to it.—G. Plekhanov, *Fundamental Problems of Marxism*, 1929, pp. 112, 117.

What a pathetic belief in dialectical jugglery, which scientifically and practically is refuted every moment of our lives! We erect a number of static categories essentially incompatible with any real *becoming*; and then, when we find we cannot fit motion into our Procrustean bed, we shout *contradiction*!

We have discussed at some length the simple derived measure known as velocity, and its relation to experienced motion, because if we once justify our view in this case it will have far-reaching effects in other parts of physics; in particular it will involve a radical departure from the existing treatment of electrical quantities and units. But it is also necessary to deal briefly with the measures known as area and volume. Let us begin by quoting Sir Oliver Lodge as an exponent of the view we propose to refute:

> When we say volume = lbh or length × breath × height, we may and should mean by l the actual length, by b the actual breadth and by h the actual height—and *not* the *number* of inches or centimetres in each; and the resulting product is then the actual volume and not any numerical estimate of it. . . . This is one of the few things on which presently I wish to dogmatise. . . . From this point of view the symbols of algebra are concrete or real physical quantities, not symbols for numbers alone.—*Easy Mathematics, chiefly Arithmetic*, 1906, p. 53.

Dogmatising is a poor substitute for argument. When we are told that Volume is actually generated by 'multiplying' the Lengths L_1, L_2, L_3, then we are being regaled with a meaningless metaphor. We know what we mean by Volume and we know what we mean by Length; our intuition shows us the unique spatial relation between a cubic Volume and its *twelve* positionally placed Edges. We also know what we mean by the multiplication of numbers. But we do *not* know what is meant when we are told to select three Edges, three linear spatial properties of the Volume, and to multiply them. Yet Sir Oliver says (p. 54), 'we may proceed without compunction to multiply together all sorts of incongruous things if we find any convenience in so doing.' He thinks L^3 is 'real and intelligible,' while L^4 is 'nonsense' and cannot appear 'in a correct end-result.' While our view is that even L^3 is nonsense and that l^4 is quite common and intelligible.

Now surface does not *mean* two-dimensionality; on the contrary, two intersecting axes already presuppose a plane. If we consider the lines in isolation and not in one and the same

space, they cannot be regarded as intersecting. The mere duality of the two lines is therefore insufficient. One is *spatially* outside the other; hence spaciness of the interrelationship is already assumed. The plane itself, we conclude, does not receive its determination from any statement about entities *in the plane*. If therefore we call the rectangle L_1L_2, we must not delude ourselves into thinking that we have obtained a plane area by any conceivable operation on two linearly spatial entities regarded simply as two specimens of their kind. We can of course regard L_1L_2 as an agreed symbol for the construction of the rectangle. But however we view it, we have already assumed the new category of area within which we operate. There is no analogy whatever with the multiplication of two numbers.

Similarly three-dimensionality has no meaning apart from body-space, while the latter has a meaning apart from dimensions.[21] Dimensions *mean* intersecting lines and have no meaning except in an independently existing space. If three-dimensionality had a meaning independently of body-space or even if it were synonymous with it, then we could think of four-dimensional space. But in fact the latter can be neither imagined nor conceived; it is a metaphor, a very useful labour-saving metaphor in spite of the nonsense written by popularisers of science. By speaking of dimensions in a purely arithmetical or analytical sense, we are enabled to use our space-intuitions as memory-aids and to employ the rich vocabulary of geometry by way of analogy. But we are not dealing with spatial magnitudes at all—no more than we are when, in the kinetic theory of gases, we speak of a representative point in n-dimensional space. We have to do with ratios or measures, pure numbers expressed in algebraic notation.

The view that the introduction of magnitude-symbols, susceptible of arithmetical operations, into physics is justified by an appeal to area and volume, is very widespread. Let us quote one of the best-known historians of science:

> A surface in geometry is conceived as having two dimensions; it is two lines, two lengths, expressed in metres for example, which are combined by multiplication. That is evidently quite an

[21] There is a childlike simplicity about M. Borel's remark: "The much-discussed question with regard to the number of dimensions in space is quite simple: space is three-dimensional because volumes are proportional to the cubes of lengths.'—*Space and Time*, 1926, p. 6.

exceptional operation ; even in geometry we cannot combine in this matter a surface with another surface nor a solid with another solid. This privilege is limited even for lengths, beyond the third power the symbol can no longer be translated into our reality. It is also quite as evident that the operation we perform on lengths is not of the same nature as those to which we subject abstract numbers. In multiplying a number by another, we never obtain anything but a number analogous with the two first, while here two lengths give us a surface, that is, something essentially different from the two factors.—E. Meyerson, *De l'explication dans les sciences*, 1921, ii. 208 f.

But even M. Meyerson has scruples. 'It seems clear,' he says, 'that such operations are not *ipso facto* legitimate. How can we conceive a weight (in kilograms) multiplied by a time (in seconds)? Is it not like multiplying metres of cloth by litres of milk?' Is it not astonishing that there should be still such confusion concerning the veriest elements of physics and mensuration? This eminent writer seriously thinks that physicists and even surveyors are occupied all day in performing operations analogous to multiplying cloth and milk and thus by a species of legerdemain producing 'something essentially different from the two factors,' a form of hybridisation exceeding the powers of the most sanguine Mendelian!

It is obvious to anyone conversant with current expositions of units and dimensions in electromagnetics that this pseudo-mystical outlook is still prevalent—even among otherwise hard-headed electrotechnicians. M. H. Abraham (i. 13*) is one of the few to utter a mild warning :

> In electricity the formula expressing Ohm's law, $V = jr$, . . . signifies only that the measured potential-difference is proportional both to the current and to the resistance. . . . Hence it is not possible to say that the potential IS the product of current and resistance, if one wishes to attach to this statement the meaning of a more or less mysterious multiplication of these magnitudes taken in themselves.

In accordance with the arguments just given, we go farther than M. Abraham. For we deny that there are any magnitudes *per se* corresponding to the derived quantities V, j, r. No doubt, there is an objective reality—say, a flow of electrons—corresponding to what we call an electric current. This is what we have called the context, the objective circumstances and processes which alone give physical significance to our algebra. But

current-*measure* is simply a number obtained by combining certain basic measures. So far as the equations of physics are concerned, V, j and r are ordinary numbers, and there cannot possibly be the smallest objection to the formula $V = jr$.

For the same reason we cannot accept without qualification the following account of the formula : rate of heat-production $= j^2r$.

> We discover that the heat evolved per second when a current of strength j passes along a wire is proportional to j^2. This means that if j is measured by means of its footrule derived from the deflection of the magnetic needle (a length) and if the heat is similarly measured by means of a length—say the expansion of a metal bar such as a thread of mercury—then when the latter measurement shows that there is four times the heat passing out, the former measurement shows that there is twice the current passing ; and so on. In the final statement the 'footrules,' both length-measurers, are switched out of the story altogether, and the relation stated as one between Rate of Heat Production and Current.—Prof. H. Levy, *Proc. Arist. Soc.*, 1937, p. 97*.

This justification is rather roundabout. The use of surrogative measures is quite a separate problem. The length of a mercury-thread to measure temperature is validated by empirical observation subsequently refined and extended. The measure may be designated 'length,' but only in a very special sense. It is not any length associated geometrically with the system, we are not at all interested in the length as such and it is only accidentally that its graduations are equidistant. We call the length the temperature because we have reason to believe that it measures a certain thermal property of the system ; and subsequently other substitutes were discovered which in some cases supersede the thermometer. Similar remarks apply to the measure of a current by means of the sine or tangent of an angle. It is rather misleading to speak of footrules being introduced and then being removed. Like other equations of physics, $h = j^2r$ is a relation between operational ratios, whose laboratory context is exceedingly complex and involves interrelation between thermal and electrical phenomena. But it is an over-simplification to speak of Current as if it were a single quasi-substantial objectification of the current-measure. This latter is, say, $j = nev$; it involves a number of discrete entities, a peculiar property of each which results in the measure we call charge, and their motional characteristic which we measure as velocity. We may, of course, use the

word Current to designate this complex statistical fact; but it is open to the same misinterpretation as the word Velocity.

We shall develop this point of view in the next chapter. Meanwhile, as a preliminary to our discussion of the many extraordinary statements made in connection with electricity, we shall give a lengthy quotation from Lord Kelvin. It is a perfect illustration of the perverted metaphysics which is so widespread in electromagnetics.

> It is interesting, not only in respect to the ultimate philosophy of metrical systems but also as full of suggestions regarding the properties of matter, to work out in detail the idea of founding the measurements of mass and force on no other foundation than the measurement of length and time. In doing so we immediately find that the square of an angular velocity is the proper measure of density or mass per unit volume; and that the fourth power of a linear velocity is the proper measure of a force. The first of these statements is readily understood by referring to Clerk Maxwell's suggestion of taking the period of revolution of a satellite revolving in a circle, close to the surface of a fixed globe of density equal to the maximum density of water, as a fundamental unit for the reckoning of time. Modify this by the independent adoption of a unit of time, and we have in it the foundation of a measurement of density, with the detail that the density of the globe is equal to $3/4\pi$ of the square of this satellite's angular velocity in radians per second, . . . It may be a hard idea to accept, but the harder it is, the more it is worth thinking of and the more instructive in regard to the properties of matter. There it is, explain it how you will, that the density of water, the density of brass, the mean density of the earth, is measured absolutely in terms of the square of an angular velocity. . . . The dimension for the reckoning of density is the square of an angular velocity on the universal-gravitation absolute system, and is therefore T^{-2}.
>
> Equally puzzling and curious is a velocity to the fourth power for the reckoning of force. . . . Now if I were to say that the weight of that piece of chalk is the fourth power of twenty miles an hour, I should be considered fit not for this place but for a place where people who have lost their senses are taken care of. I suppose almost everyone present would think it simple idiocy, if I were to say that the weight of that piece of chalk is the fourth power of seven or eight yards per hour; yet it would be perfectly good sense.—Kelvin, ii. 104. Cf. Maxwell in 1877 (Campbell-Garnett, p. 400).

Let us see what is the scientific content of this specimen of the ultimate philosophy of metrology. Suppose a particle revolves, under gravitational attraction, with angular velocity ω round a

homogeneous sphere (density ρ and radius r), close to the surface. The central acceleration is

$$\omega^2 r = \gamma m/r^2 = 4\pi\gamma/3 \,.\, r\rho,$$

so that $\omega^2 = 4\pi\gamma\rho/3$. And if $T = 2\pi/\omega$ is the period, we have

$$T = (3\pi/\gamma\rho)^{\frac{1}{2}}.$$

Theoretically, there is no objection to taking this, instead of a fraction of the sidereal day, as our unit of time. The question will become practical when someone produces the globe and the satellite! But what is mysterious in this equation, which is not already contained in the pendulum-formula $T = 2\pi(l/g)^{\frac{1}{2}}$? And how is the equation $\rho = 3\omega^2/4\pi\gamma$ 'a hard idea to accept'? The fact that the measure-numbers of physics are interrelated, ought by this time to be rather commonplace.

Suppose further that a particle of mass m revolves in a circle of radius r round a particle of equal mass. Its acceleration is $v^2/r = \gamma m/r^2$. Hence the force acting on it is

$$f = \gamma m^2/r^2 = v^4/\gamma.$$

Here, in virtue of the law of gravitation, we have another interrelation between the numbers f, v and γ. Good physics and good sense. Now by 'the weight of that piece of chalk' we may be alluding to a certain phenomenon familiar to us by our muscular sense and by various experiments. But when we employ the symbol w, we mean the ordinary number resulting from a certain comparison. And similarly by 'twenty miles an hour' we mean the number 20, which results from certain metrical operations. And it may happen that we find $w = 20^4$. If the body is falling against a resistance proportional to the square of the speed, it will ultimately tend to move uniformly so that $w = kv^2$. In fact there is as much, or as little, 'puzzling and curious' in saying that the resistance is kv^2 as in saying that, in the former case, the force is v^4/γ. The mystification sets in when we begin to misinterpret these numbers as complex qualitative happenings miraculously susceptible to arithmetical operations such as raising to the fourth power. It is precisely the failure to recognise the symbols of physics as ordinary numbers, which has led electricians into such a quagmire of futile and meaningless metaphysics. We need not be too surprised, for have we not just heard the great Kelvin unwittingly talking nonsense?

We shall illustrate from electromagnetics the point we have

just made concerning velocity. We can synthetically express both Lorentz's and Ritz's theories by saying that the force, in elst measure, between two moving charges is given by an extension of Coulomb's law :

$$ee'/r^2 \,.\, f(v/c).$$

Putting $e'\mathbf{v}' = j\mathbf{ds}$ in formula (12.20) and integrating, we can assert that we have *deduced* from the above force-formula that the 'magnetic intensity,' in mag measure, is given by

$$\mathbf{H} = j/c \,.\, \int V\mathbf{dsr}/r^3.$$

A particular case is the formula for the magnetic field at the centre of a circular circuit :

$$H = 2\pi j/cr.$$

All this argument is straightforward and has already been given at length. There is no doubt whatever that in these formulae c is some critical velocity ; and a comparison with Hertz's experiments has shown that $c = 3 \,.\, 10^{10}$, the velocity of light.

Though we are reversing the historical (but not the logical) order, we can now say that we decide on a new measure of current, the elm measure $j' = j/c$. Accordingly, the last formula becomes $H = 2\pi j'/r$. Thus we have two units of current whose ratio is c, as has also been verified independently.

Incidentally we may interpolate the observation that this is no argument for, or peculiarity of, Maxwell's theory ; it holds equally on Ritz's theory. Hence the usual contention is inadmissible.

The agreement or disagreement of the values furnishes a test of the electromagnetic theory of light. . . . Our theory asserts that these two quantities are equal and assigns a physical reason for this equality.—Maxwell, ii. 436.

During Maxwell's time it was realised that the ratio of the elm to the elst unit has the dimensions of a velocity. This consideration lent some support to Maxwell's views from the purely theoretical side of the subject. But ten years after Maxwell's death, electromagnetic waves were actually detected, their velocity calculated, and the results of experiment found to agree with the predictions of his theory.—D. M. Turner, *Makers of Science : Electricity and Magnetism*, 1927, p. 136.

It is a consequence of Maxwell's theory that every electromagnetic

disturbance in vacuum is propagated with a velocity equal to the ratio of the elm and elst units of charge. If then we succeed in finding this ratio and in measuring this velocity, a comparison of these two quantities will allow us to refute or to confirm Maxwell's theory.—L. Bloch, p. 319.

If the experimentally measured velocity of light and the experimentally measured ratio of the units are found to be the same, this agreement constitutes strong evidence in favour of Maxwell's assumption of displacement-currents and the electromagnetic nature of light.—G. Harnwell and J. Livingood, *Experimental Atomic Physics*, 1933, p. 6.

To the unsophisticated person it seems quite simple and natural that we should use two different measures for charge, one being $3 \, . \, 10^{10}$ times the other, in different ranges of phenomena; it does not appear to be anything more peculiar than the fact that we have different measures, varying from microns to light-years, for length. Nevertheless there is a great outcry, a regular chorus of objections.

The electrostatic system of measurement . . . is independent of and incompatible with the electromagnetic system—Maxwell, iii. 569.

It is not to be supposed that we can long go on with two distinct systems of units, the electrostatic and the electromagnetic, and two distinct sets of dimensions for the same quantities.—Sir O. Lodge, i. 404.

It seems absurd that there should be two different units of electricity; still more absurd that one unit should be thirty thousand million centimetres per second greater than the other.—S. P. Thompson, p. 352.

What in the world has the mutual attraction of two charged spheres got to do with the square of the velocity of light ?—Pohl-Roos, p. 15.

We cannot remain without misgivings when we find the quantity c, the velocity of light, a factor in the dimensional expressions. . . . The author has for long endeavoured to find in relativity an answer to the riddle which has intrigued physicists and others for the last 60 or 70 years.—Lanchester, p. 281.

The two rival systems of measuring electrical quantities were developed at a time when the relationship between electric and magnetic forces was not clearly understood. Now that more is known about this relationship, it is highly desirable that only one kind of system should be employed.—Pilley, p. 192.

After we have raised charge to the rank of an independent unit, it is no longer necessary to have it in the ratio c to itself. This statement, with which we have frightened generations of students, is at least very undidactic.—Sommerfeld, ii. 817.

It seems almost incredible that such an outcry could be raised over such an elementary matter. It is no more 'absurd' to have two measures for charge than it is to express length in centimetres and feet. There is as little incompatibility as there is between measuring force in pounds and dynes. If successive professors have been scrupulous, if generations of students have been frightened, the reason is very simple: they have failed to grasp the very meaning of the symbols of physical science. In expressing Coulomb's law $f = qq'/\alpha r^2$, we sometimes take $\alpha = 1$ and sometimes we put $\alpha = c^{-2}$, where c is both the ratio of the units and the velocity of light. If it happened to be convenient, we could take c to be the velocity of sound in air, or Young's modulus for steel. What on earth has c to do with the force between two stationary point-charges? Nothing whatever; it is *we* who have inserted the number c. How can one unit be $3 \cdot 10^{10}$ *centimetres per second* times the other? The answer is that the italicised words are a ridiculous interpolation. For $c = 3 \cdot 10^{10}$, when we use the c.g.s. system; it is false to say that $c = 3 \cdot 10^{10}$ cm./sec. if this last appendage is regarded as a 'qualitative' multiplier. Our rather lengthy excursus on velocity-measure has not been in vain if it serves as the first step towards clarifying the question of electrical units.

Incidentally we have shown that the question of employing so-called rational or rationalised units is purely a matter of practical convenience (or perhaps inconvenience), and has not the remotest theoretical significance. It is nothing more or less than taking $\alpha = 4\pi$. Heaviside, of course, considered every other value quite irrational and absurd.

> What was more natural than to make the expression of the law as simple as possible by giving the constant α the value unity—if indeed it were thought of at all? Our ancestors could not see into the future—that is to say, beyond their noses—and perceive that this system would work out absurdly.—Heaviside, iii. 117.

And so he proposed (iii. 119) 'the cure of the disease by proper measure of the strength of sources,' by introducing 'the natural measure' of charge. Or, in plain language, Heaviside demanded that α should be made 4π; and if anyone objected, well, he was an imbecile.

3. Measure-Ratios.

Let us consider a magnitude—say a Length (Fig. 72)—which we designate by A, and two conspecific units to which we refer by the letters B_1 and B_2. The measure of A in terms of B_1 as unit is $l_1 = A/B_1$; its measure with B_2 as unit is $l_2 = A/B_2$. And $L = l_1/l_2$ will be called the *measure-ratio*, that is, the ratio of the measures of the magnitude A with reference to the two respective units B_1 and B_2. It is to be particularly noted that while A, B, C will be temporarily employed to designate magnitudes, the letter L—and similarly M and T—will henceforth be used to signify *an ordinary number*. It is also convenient, but in no way necessary, to measure the two units in terms of a third conspecific magnitude C which we shall call the 'standard.' These measures are $l_1' = B_1/C$ and $l_2' = B_2/C$. We then have

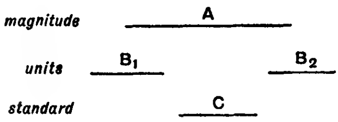

Fig. 72.

$$L = \frac{l_1}{l_2} = \frac{A/B_1}{A/B_2} = \frac{B_2}{B_1} = \frac{B_2/C}{B_1/C} = \frac{l_2'}{l_1'}. \tag{14.3}$$

Hence L, being equal to the inverse ratio of the units, is independent of the particular specimen A of whose measures L is the ratio. That is, for given units 1 and 2, the number L is the same for all members of the A-class, i.e. for all Lengths. Obviously the same holds for Mass and Time; and we need not assume any magnitude Mass.

Suppose now that we change from set 1 to set 2 of units of length, mass and time. Our measures l_1, m_1, t_1 become l_2, m_2, t_2; where $l_1 = Ll_2$, $m_1 = Mm_2$, $t_1 = Tt_2$. The capital letters denote ordinary numbers, namely, measure-ratios; they are independent of the particular magnitude or quantity measured, for each is the inverse ratio of the respective units 1 and 2. The measures of derived quantities are also changed. If V is the measure-ratio of velocity, we have

$$V = \frac{v_1}{v_2} = \frac{l_1/t_1}{l_2/t_2} = \frac{l_1/l_2}{t_1/t_2} = \frac{L}{T}.$$

This we shall call a *logometric* formula.[22] It constitutes the

[22] From λόγος (ratio) and μέτρον (measure). It is therefore merely a more euphonious form of the adjective measure-rational.

sturdy scientific reality behind those mysterious things called 'dimensional formulae.' It is the pragmatic or working equivalent of the abstruse equation for unit Velocity; it is literally identical with (14.2), but the meaning is completely transformed. All the capital letters stand for 'mere' numbers; they need not be enclosed in square brackets to defend their virginal qualitativeness against the assaults of vulgar algebra; they are already algebraic. Sometimes indeed we shall use square brackets, but only for the purely alphabetical reason that we may be short of letters. We may prefer to call some measure by a capital letter such as H, and then $[H]$ can conveniently stand for H_1/H_2. Or, by reason of unfamiliarity or ambiguity, we may wish to avoid the use of Greek capitals; hence we may use $[\beta]$ for β_1/β_2. Similarly, we may change the letter, e.g. using D for the density-ratio ρ_1/ρ_2. But measure-ratios have no connection with square-bracket metaphysics or with the pseudo-algebraic manipulation of magnitudes.[23]

What we have done for velocity is obviously applicable to the other quantities of mechanics, as will be clear from this brief table.

Quantity	*Equation*	*Logometric formula*
velocity	$v = l/t$ or $\delta l/\delta t$	$V = L/T$
acceleration	$a = \delta v/\delta t$	$A = V/T = L/T^2$
force	$f = ma$	$F = ML/T^2$
energy	$w = mv^2/2$	$W = ML^2/T^2$
density	$\rho = m/v$	$D = M/L^3$
viscosity	force/area $= \mu dv/dy$	$[\mu] = M/LT = DVL$

(14.4)

It is clear, therefore, that our logometric formulae are identical with the usual dimensional formulae—minus their shockingly

[23] 'To show that there is question only of *qualitative* equations, it is usual to employ square brackets.'—Schaefer, ii. 7. 'The student might think that he was doing algebra and that L represented a number. . . . To avoid this error, it is customary to put the dimensions within squared brackets.'—R. de Villamil, *Rational Mechanics*, 1928, p. 18. 'The three fundamental dimensions are represented by symbols in a light weight of black type—thus L, M, T—so ensuring that they shall not be mistaken for symbols denoting algebraic quantities; they have no connection with the system of units employed. These dimensional symbols are used only to express the nature or dimensional constitution of the entities they serve.'—Lanchester, p. xv.

bad metaphysics. We have saved their scientific content while cutting their absurd substructure away; and we celebrate the victory by changing their name. Too many bad associations cling to the term 'dimensions,' which in any case has another (spatial) connotation. The term 'measure-ratio' is simple and direct.[24]

In any case our view is identical with that of Fourier who, with a slight change of notation, wrote in 1822 [25]:

> In the analytical theory of heat, every equation expresses a necessary relation between the existing magnitudes l, t, θ, c, q, λ. This relation depends in no respect on the choice of the unit of length, which from its very nature is contingent; that is to say, if we took a different unit to measure the linear dimensions, the equation would still be the same. Suppose then the unit of length to be changed, and its second value to be equal to the first divided by L. Any quantity whatever l, which in the equation represents a certain line AB and which consequently denotes a certain number of times the unit of length, becomes Ll corresponding to the same length AB.

Fourier assumes that the symbols of physics denote ratios or measures. His L, T, Θ (which he calls m, n, p) denote measure-ratios; he had no occasion to consider M, though later on (p. 130) he considers a change in the 'unit of weight.' It is thus clear that we are following the writer to whom is due 'the first table of dimensions,' as Maxwell and Jenkin tell us (p. 89). What is not noticed, even by Maxwell himself, is that Fourier's simple theory is altogether different from the objectionable explanation of dimensions which was given by Maxwell and is now generally followed.

By reverting to the elementary common-sense treatment of Fourier, we have accordingly reinterpreted the current dimensional formulae and got rid of the bad metaphysics prevalent

[24] Indeed the meaning of the term 'dimension' is often quite uncertain. After reading Bridgman's *Dimensional Analysis* I am still unable to say what he means by it. Elsewhere (in Perry-Calcott, *Chemical Engineers' Handbook*, 1934, p. 246) he tells us: 'To obtain the number which represents the velocity of a given object, we divide the number which measures the distance it has passed over by the time required to pass over that distance, i.e. velocity has the dimensions L/T.' That is, $V = L/T$ is identical with $v = l/t$!

[25] *Analytical Theory of Heat*, § 161, Eng. trans. Freeman, 1878, p. 128*. It is preferable to talk of measure-ratios rather than inverse ratios of units. This will become clearer when we come to similarity.

since Maxwell. We can already draw a few important conclusions. In the first place, a measure such as l_1 does not 'have' a measure-ratio in any intelligible sense; in current phraseology, a physical quantity does not 'possess' dimensions. One measure does not have a measure-ratio, which is the ratio of two measures; in addition to l_1 we require l_2, which may be equal to or greater or less than l_1. The particular value of L is arbitrary, it depends entirely on the second unit which we select. We may put $L = 1$, i.e. $l_1 = l_2$; that is, we have decided *not* to change our unit of length. We can make any quantity 'dimensionless'; the statement sounds alarming, but is really quite trivial. If we refuse to budge, if we decline to use any other units, then there is no measure-ratio at all except unity, there are no dimensions! Take for instance Coulomb's law in elst measure ($f = qq'/r^2$) and change our measures of length, mass and time in the ratios L, M, T. Then the measure of charge is changed in the ratio Q given by

$$Q^2/L^2 = F = ML/T^2,$$

or

$$Q = L^{3/2}M^{\frac{1}{2}}T, \tag{14.5}$$

where all the letters represent ordinary numbers. There is no difficulty in making $Q = 1$; all we require is that $L^3M = T^2$ or, as a particular case, $L = M = T = 1$. It is not true to say, with Sir J. J. Thomson (ii. 343), that 'the dimensions of electrical quantities are a matter of definition and depend entirely upon the system of units we adopt.' They depend entirely on the *changes* we wish to make in the system of units with which we started. There is no point in saying with Sir James Jeans (p. 15) that 'these dimensions are merely apparent and not in any sense real,' or in saying with Carvallo (p. 494) that they are 'fictitious dimensions' without 'physical meaning.' They are just as real, no more and no less, as Coulomb's law. The idea that measure-ratios must somehow throw light on 'the nature' of things must be regarded as an exploded supersitition.[26] It is also a relic of unintelligible metaphysics to declare that 'it seems absurd that the dimensions of a unit of electricity should have fractional

[26] 'For those investigators whose activity lies primarily in applied science it is both simpler and safer to consider the physical dimensions of a quantity as inherent in its nature although perhaps unknown to us.'—Karapetoff, p. 724. So the practical man is advised to cling to the esoteric superstition!

powers, since such quantities as $M^{\frac{1}{2}}$ and $L^{3/2}$ are meaningless.'[27] One might as well say that $y^{\frac{1}{2}}$ or $x^{3/2}$ is 'meaningless' in algebra! The hideous nightmare of 'dimensions' disappears, when we wake up to the simple fact that L, M, T are merely measure-ratios.

There is no difficulty in finding the general formula for the measure-ratio of a derived quantity. It will be observed from the examples already given that in general

$$q_1/q_2 = Q = L^x M^y T^z. \tag{14.5a}$$

We shall now see the advantage of our assumed third or 'standard' set of units; for $L = l'_2/l'_1$, $M = m'_2/m'_1$, $T = t'_2/t'_1$. Whence

$$q_1 l_1'^x m_1'^y t_1'^z = q_2 l_2'^x m_2'^y t_2'^z \tag{14.6}$$

We can now drop the dashes as there is no danger of confusion; the l's and t's denote the measures of the units in the standard system. If the units in which q is measured are cm., gram and sec. and if q' is the measure when the units are a cm., b gram, c sec., then

$$q = q' a^x b^y c^z. \tag{14.6a}$$

As a particular case of (14.6) we have

$$v_1 l_1/t_1 = v_2 l_2/t_2.$$

Suppose the set 1 is ft. and sec., the set 2 is mile and hour, and the standard set is yard and minute. (To be consistent we should use capital letters for these *magnitudes*, but we conform to the ordinary notation.) Then

$$v_1 \frac{\text{ft./yard}}{\text{sec./min.}} = v_2 \frac{\text{mile/yard}}{\text{hr./min.}} = v_2 \frac{5280 \text{ ft./yard}}{3600 \text{ sec./min.}}.$$

Now ft./yard and sec./min. are ordinary numbers—namely, 1/3 and 1/60—so we can cancel them from both sides of the equation. We obtain $v_1 = (88/60)v_2$, which is an ordinary algebraic

[27] S. P. Thompson, p. 352. 'While hitherto only integral exponents occurred, we now meet with fractional—which in reality has no meaning.'—F. Auerbach, *Die Methoden der theoretischen Physik*, 1925, p. 12. 'This dimensional formula for Q is very complicated, and its interpretation is made difficult owing to the fractional index of M, which seems irrational.'—E. Fournier d'Albe, *The Electron Theory*, 1906, p. 296. 'We can attach no meaning to M^{-1}, the inverse of a mass. . . . We can attach no meaning to the quantities $[M^{\frac{1}{2}}]$ and $[L^{3/2}]$.'—Starling, (p. 385). 'It does not seem possible to attach any physical meaning whatever to a fractional dimension.'—H. Heckstall-Smith, *Intermediate Electrical Theory*, 1932, p. 471.

equation without any qualitative appendages. But observe that the yard and minute did not enter into the business at all; we should have got the same result if we had taken any other standard set. In particular, if we choose ft. and sec., we have

$$v_1 = v_2 \frac{\text{mile/ft.}}{\text{hr./sec.}} = v_2 \frac{5280}{3600} = \frac{88}{60} v_2.$$

The usual way of expressing this is as follows [28]:

$$v_1 \frac{\text{ft.}}{\text{sec.}} = v_2 \frac{\text{mile}}{\text{hr.}} = \frac{60}{88} v_1 \frac{\text{mile}}{\text{hr.}}$$

As ordinarily accepted, this equation is either wrong or meaningless. But it is capable of being saved (if it is worth it) by a re-interpretation. *Let us interpret the word-symbols as measures*; so that, for instance, ft. means a foot measured in the standard (unspecified) system, i.e. it means l_1 or, as a particular case, foot/yard.

In this sense v_1 ft./sec. $= v_2$ mile/hr. is only another way for writing $v_1 l_1/t_1 = v_2 l_2/t_2$. But we do not seem to have gained anything by this, for we must use this new interpretation explicitly in order to prove $v_1 = (88/60)v_2$. It would therefore appear to be better to take the current usage as merely containing a qualitative or operational direction. Thus $v = 16$ cm./sec. would mean $v = 16$ when Length is measured in cm. and Time in sec. The phrase has the advantage of suggesting the really practical transformation-formula $v_1 l_1/t_1 = v_2 l_2/t_2$. But it remains painfully liable to absurd interpretations. Practically every student who uses it fancies that 16 is to be multiplied by some qualitative entity or unit called cm./sec., which somehow is not just 1/1. The proper remedy is to write $v = 16$, and, separated from this, to write cm./sec. in the margin to give an indication as to how the number 16 has been reached, namely,

[28] Bridgman (Percy-Calcott, *Chemical Engineers' Handbook*, 1934, p. 247) gives the following 'symbolic form':

$$\text{velocity} = \frac{88 \text{ ft.}}{1 \text{ sec.}} = \frac{88/5280 \text{ . miles}}{1/3600 \text{ . hr.}} = 60 \frac{\text{miles}}{\text{hr.}}$$

Similarly Planck (*General Mechanics*, 1932, p. 8), using square brackets, gives:

$$20 \left[\frac{\text{cm.}}{\text{sec.}}\right] = 20 \left[\frac{1/100 \text{ . metre}}{1/60 \text{ . min.}}\right] = 12 \left[\frac{\text{metre}}{\text{minute}}\right].$$

Thus the appalling difficulties inherent in the notation Length/Time are supposed to be overcome by a reference to symbolism or by using brackets!

by dividing centimetre-measure by second-measure. A similar remark applies to all other derived or compound quantities.

A derived quantity (q) can be expressed as a function of the basic measures, for which we take, say, three independently variable quantities l, m, t; i.e. $q=f(l, m, t)$. Let us now determine the form of this function from the assumption that the measure-ratio Q, like V and the other measure-ratios of mechanics, is independent of l, m, t and depends only on the measure-ratios L, M, T. That is, we take

$$Q=f(Ll, Mm, Tt)/f(l, m, t)=\varphi(L, M, T).$$

Keeping $M=T=1$, let us change l successively by the factors L and L':

$$\begin{aligned} f(LL'l, m, t) &= \varphi(L, 1, 1) \,.\, f(L'l, m, t) \\ &= \varphi(L, 1, 1) \,.\, \varphi(L', 1, 1) \,.\, f(l, m, t). \end{aligned}$$

But since the double change L followed by L' must give the same result as the single change LL', we also have

$$f(LL'l, m, t)=\varphi(LL', 1, 1) \,.\, f(l, m, t).$$

Hence

$$\varphi(LL', 1, 1)=\varphi(L, 1, 1) \,.\, \varphi(L', 1, 1).$$

The only solution of this is $\varphi(L, 1, 1)=L^x$. Similarly

$$\varphi(1, M, 1)=M^y, \quad \varphi(1, 1, T)=T^z.$$

Hence finally

$$Q=L^xM^yT^z. \tag{12.7}$$

It follows at once that, if this formula [29] is true, the ratio of the measures (q and q') of any two specimens of a derived quantity is independent of the units employed. For

$$q_1/q_2=L^xM^yT^z=q_1'/q_2',$$

so that

$$q_1/q_1'=q_2/q_2'.$$

We can also approach this result in another way. The function $q=f(l, m, t)$ is supposed to be continuous. Now Weierstrass [30] proved the fundamental proposition that a continuous function

[29] A more cumbrous proof will be found in Bridgman, i. 21 f.

[30] See E. Borel, *Leçons sur les fonctions de variables réelles*, 1905, p. 50; or Picard, *Leçons sur quelques types simples d'équations aux dérivées partielles*, 1927, p. 18.

can, to any arbitrary approximation, be represented by a series of polynomials. Hence we can put

$$q = \Sigma a l^x m^y t^z$$

and

$$Qq = \Sigma a L^x M^y T^z l^x m^y t^z.$$

Since this is to hold for all changes of units, i.e. for arbitrary values of L, M, T, we must have

$$Q = L^x M^y T^z$$

so that every term has the same measure-ratio. This is the so-called principle of dimensional homogeneity. Suppose we have any equation in general unspecified units

$$f(l_1, m_1, t_1\,;\ l_2, m_2, t_2\,;\ \ldots) = 0.$$

The formula holds independently of the units. Hence

$$f(Ll_1, m_1, t_1\,;\ Ll_2, m_2, t_2\,;\ \ldots) = 0$$

for *all* values of the multiplier L. This can be true only if some power of L is a factor, so that

$$f(Ll_1, \ldots) = L^x f(l_1, \ldots).$$

Similarly for M and T. Hence

$$f(Ll_1, Mm_1, Tt_1, \ldots) = L^x M^y T^z f(l_1, m_1, t_1, \ldots).$$

That is, each term has the same measure-ratio. This is all that is stated by the principle of logometric homogeneity. It contains no implication whatever that the symbols denote anything but pure numbers. We must therefore reject the usual statement and alleged proof, as typified in these quotations :

> We can add any number of lengths or of times or of velocities ; but to add the numerical values of a length and a time or a length and a volume, is a meaningless act so far as rational physics is concerned. This can be stated as a positive general principle in the following words : In any physical equation every term must have the same dimensions.—Prof. A. W. Porter, *Enc. Brit.* 22 (1929[14]) 853.

> Since the mathematical formulation of any physical law is a statement of equality or relationship between physical quantities of the like nature, . . . it follows that all the terms in any equation having a physical significance must necessarily have identical dimensions.—Prof. H. Levy, *Dict. Applied Physics*, 1 (1922) 82.

> The dimensions of a quantity may be best regarded, I believe, as a shorthand statement of the definition of that kind of quantity in terms of certain fundamental kinds of quantity, and hence also as an expression of the essential physical nature of the quantity in question.—R. Tolman, PR 8 (1916) 9.

The principle of dimensional homogeneity . . . merely expresses the obvious necessity that all the terms in an equation connecting physical quantities shall have the same physical nature.—R. Tolman, PR 9 (1917) 251.

It is not true to say that $l + t$ is 'meaningless,' it is as significant as the $x + y$ of algebra. But when we change the units it becomes $Ll + Tt$; hence it cannot occur in an algebraic relation which is valid for arbitrary values of L and T. The principle merely involves simple algebra, it requires no insight into the 'nature' or 'kind' of any magnitudes save ordinary numbers. Thus explained, it is intelligible to the veriest tyro in physics.

Changing our notation, let us call the basic quantities, supposed to be three, x_1, x_2, x_3 instead of l, m, t. Let q_1, q_2, q_3 be any derived quantities, such that the measure-ratio

$$Q_1 = X_1^{a_{11}} X_2^{a_{12}} X_3^{a_{13}}$$

or

$$\log Q_1 = a_{11} \log X_1 + a_{12} \log X_2 + a_{13} \log X_3,$$

and similarly

$$\log Q_2 = a_{21} \log X_1 + a_{22} \log X_2 + a_{23} \log X_3,$$
$$\log Q_3 = a_{31} \log X_1 + a_{32} \log X_2 + a_{33} \log X_3.$$

The solution is

$$\log X_1 = c_{11} \log Q_1 + c_{12} \log Q_2 + c_{13} \log Q_3$$

or

$$X_1 = Q_1^{c_{11}} Q_2^{c_{12}} Q_3^{c_{13}},$$

with corresponding expressions for X_2 and X_3, where

$$c_{11} = \begin{vmatrix} a_{22} & a_{23} \\ a_{32} & a_{33} \end{vmatrix} \Big/ \Delta,$$

$$c_{12} = \begin{vmatrix} a_{13} & a_{12} \\ a_{33} & a_{32} \end{vmatrix} \Big/ \Delta,$$

$$c_{13} = \begin{vmatrix} a_{12} & a_{13} \\ a_{22} & a_{23} \end{vmatrix} \Big/ \Delta,$$

$$\Delta \equiv | \, a_{11} \, a_{22} \, a_{33} \, |.$$

Hence, if Δ is not zero, we can express X_1, X_2, X_3 in terms of Q_1, Q_2, Q_3. Which we can express as follows: The condition that three derived quantities, depending on three basic, can be used as a *probasic* set, is $\Delta \neq 0$. Assuming this condition satisfied, if q is any fourth quantity, its measure-ratio is

$$Q = X_1^{b_1} X_2^{b_2} X_3^{b_3} = Q_1^{d_1} Q_2^{d_2} Q_3^{d_3},$$

where

$$d_1 = b_1c_{11} + b_2c_{21} + b_3c_{31}$$

$$= \begin{vmatrix} b_1 & a_{12} & a_{13} \\ b_2 & a_{22} & a_{23} \\ b_3 & a_{32} & a_{33} \end{vmatrix} \div \begin{vmatrix} a_{11} & a_{12} & a_{13} \\ a_{21} & a_{22} & a_{23} \\ a_{31} & a_{32} & a_{33} \end{vmatrix}.$$

That is, $q/q_1^{d_1}\,q_2^{d_2}\,q_3^{d_3}$ has the same value in both sets of units, it is *tautometric* (' dimensionless '). In general ($\Delta \neq 0$), from four derived quantities depending on three basic, one tautometric product may be formed; and n quantities give $n-3$ tautometric products. We conclude then, for the case of three basic quantities, that

$$\Delta \neq 0, \tag{14.8}$$

i.e. the determinant is not zero, is the condition to be satisfied by three derived quantities q_1, q_2, q_3, so that, if q is any fourth derived quantity, we can form the tautometric product

$$q/q_1^{d_1}\,q_2^{d_2}\,q_3^{d_3}.$$

We can say that these three quantities constitute a probasic set, i.e. can be substituted for the basic quantities in setting up logometric formulae.

We now show that

$$\Delta = 0 \tag{14.9}$$

is the condition that three derived quantities, depending on three basic, should form a tautometric product. As before

$$Q_1 = X_1^{a_{11}}\,X_2^{a_{12}}\,X_3^{a_{13}},$$
$$Q_2 = X_1^{a_{21}}\,X_2^{a_{22}}\,X_3^{a_{23}},$$
$$Q_3 = X_1^{a_{31}}\,X_2^{a_{32}}\,X_3^{a_{33}}.$$

Assuming $Q_1 = Q_2^l Q_3^m$ and equating indices, we have

$$a_{11} = la_{21} + ma_{31},$$
$$a_{12} = la_{22} + ma_{32},$$
$$a_{13} = la_{23} + ma_{33}.$$

Eliminating l and m, we find

$$\Delta \equiv \begin{vmatrix} a_{11} & a_{12} & a_{13} \\ a_{21} & a_{22} & a_{23} \\ a_{31} & a_{32} & a_{33} \end{vmatrix} = 0.$$

Hence the condition that the product $q_1/q_2^l q_3^m$ should be tautometric, is $\Delta = 0$.

Let $F(q_1, \ldots q_n) = 0$ be a general equation between n quantities. By dividing F by one of its terms, we can always introduce unity as one term and thus express the equation in the form : $f(q_1, \ldots q_n) = 1$. Now if the relation is independent of the units employed,

$$f(q_1, \ldots q_n) = f(Q_1 q_1, \ldots Q_n q_n) = 1.$$

In other words, f is a tautometric function, having the value unity for all sets of units. Without offering any elaborate mathematical proof, we can easily see that such a function must be a function of tautometric products ; assuming that each term in the original equation has the same measure-ratio, each term must have been made tautometric when we divided by one of the terms. That is, we can express the relation in the form

$$\varphi(p_4, \ldots p_n) = 0. \tag{14.10}$$

This is the fundamental product-theorem, which is of such importance in physics.[31]

Let us illustrate its application by considering the flow of liquid through a pipe (diameter d, length l). The pressure-gradient is $p_{12} \equiv (p_1 - p_2)/l$, the viscosity μ, the density ρ, the mean velocity at a section is v. Assume that no other quantities are concerned, that is,

$$f(p_{12}, \mu, \rho, v, d) = 0.$$

These five quantities give two tautometric products : [31a]

$$P \equiv p_{12}/(\rho v^2/2d), \quad R \equiv \rho v d/\mu.$$

From (14.10) we conclude : $F(P, R) = 1$ or

$$P = \varphi\,(R). \tag{14.10a}$$

In the case of laminar flow (R small), Stokes in 1847 gave a proof of Poiseuille's empirical law, which will be found in elementary text-books of hydrodynamics, namely, $q = \pi p_{12} r^4/8\mu$, where $q = \pi r^2 v$ is the rate of flow, $r = d/2$ being the radius. That is,

$$P = 64/R.$$

[31] It was first given by Vaschy (p. 13) in 1897. A rigid purely mathematical proof was given (in Russian) by A. Federmann in *Izvyestiya*, 16 (1911) 124–136, Dr. E. Buckingham enunciated it in PR 4 (1919) 346 f., and called it the Π Theorem. a term often applied to it to-day.

[31a] P is usually designated λ, the 2 being inserted merely for a certain convenience in hydraulics. R is called 'Reynolds's number.' Strictly, the tautometric product d/l should also occur ; but when it is sufficiently small, it is found to be without appreciable influence.

When R exceeds a certain number, about 2000, the flow is turbulent ; and theory is of little avail. But we still have the equation (14.10a). Hence if we plot P against R, the points should all fall on one curve, even if we vary the fluid and the size of pipe. This is found to be the case for smooth pipes. If we can plot such a curve, the exact analytical expression of the function φ is not so important. Various suggestions for such a formula, applicable to the turbulent régime, have been made. An excellent but forgotten formula was given in 1911 by Menneret [31b] :

$$P = \varphi(R) = 64/R. \quad [1 + a(Q-1)^n],$$

where, if R_c denotes the critical value when turbulence sets in, $Q = R/R_c$; and a and n are constants which Menneret took to be $a = 1.413$, $n = 0.735$. When $R = R_c$, the formula coincides with Poiseuille's. When R is very large, it approximates to

$$P = 64a/R_c^n R^{1-n} = 0.339/R^{0 \cdot 265},$$

on inserting Menneret's values for a and n and on putting $R_c = 2000$. This is practically identical with the well-known formula of Blasius (1912)

$$P = 0.316/R^{0 \cdot 25}$$

or that of Nikuradse

$$P = 0.0032 + 0.221/R^{0 \cdot 237}.$$

But for very large R ($>$ 150,000), P tends to a constant value. A more general formula, with a theoretical basis, is that verified by Nikuradse [31c] :

$$P^{-\frac{1}{2}} = 2 \log (RP^{\frac{1}{2}}) - 0.8.$$

When the pipe is rough, we can approximately take the roughness into account by adding the tautometric product $S \equiv c/d$, where c is the height of a protuberance or rugosity, so that

$$P = \varphi(R, S).$$

This has been excellently verified by Nikuradse's experiments on artificially prepared sand surfaces with $1/S$ varying from 7.5 to 254. (See the graph in Bakhmeteff, p. 35).

This example indicates how far, without any appeal to physical theory, we can go in coordinating and synthesising experimental

[31b] *Mouvement oscillatoire et mouvement uniforme des liquides dans les tubes cylindriques*, Grenoble, 1911.

[31c] Barkmeteff, *The Mechanics of Turbulent Flow*, Princeton, 1936, p. 84.

data merely by general metrical considerations. We propose now to consider briefly the case of *four* basic quantities. The extension of our previous results to this case is obvious. For example : In general, $n - 4$ tautometric products can be formed from n derived quantities. The fourth basic quantity with which we are concerned is *temperature*. Temperature may be, and usually is, measured surrogatively by a length, i.e. the height of a thermometric column. But this no more implies that Temperature *is* Length than the reading of a clock implies that Time is an Angle. The length involved is not one of the spatial dimensions of the system, it is an independent variable ; just as the motion of the clock-hand is not the motion of one of the elements of the system. Formula (14.5a) now becomes

$$Q = L^x M^y T^z \Theta^w. \tag{14.10b}$$

The definition of heat-measure is

$$q = ms\theta,$$

where m is the mass of the body heated through a temperature-interval θ. The factor s—specific heat or thermal capacitance—is arbitrary in absolute value. So the convention is universally adopted of taking the specific heat of water to be unity. (If we wish to be meticulously accurate, we must add : at 15° C.) Then the quantity of heat necessary to raise an equal mass of water through the same temperature-interval is given by : $q_0 = m\theta$. That is, specific heat is made tautometric : $s = q/q_0$. In practical questions of units we put $S = 1$ and we take the measure-ratio of heat to be

$$Q = M\,\Theta.$$

Suppose, for example, we wish to find the relation between the British thermal unit and the gram-calorie. We have

$$q_1\, m_1\, \theta_1 = q_2\, m_2\, \theta_2,$$

or

$$\text{B.T.U.} \times (\text{mass of 1 lb.}) \times \text{fahr.} = \text{cal.} \times \text{gram} \times \text{cent.}$$

That is,

$$\begin{aligned} \frac{\text{B.T.U}}{\text{cal.}} &= \frac{\text{gram}}{\text{mass of 1 lb.}} \cdot \frac{\text{cent.}}{\text{fahr.}} \\ &= 453.6 \qquad \times 5/9. \\ &= 252. \end{aligned}$$

Underlying this treatment is the convenient convention that specific heats are to remain unchanged. But, it is important to observe, there is nothing obligatory or apodictic about this convention; it is logically posterior to, and independent of, the definition of heat-measure. And, if we wish to make the most general change of units, we must ignore this *post factum* convention. The agreed fact that in practice we do not change the measure of specific heat—we never, for example, take $s = 3.5$ for water (at 15° C.)—is irrelevant when our object is to find the most general form of the tautometric products involved in a thermal equation. If s is the specific heat per unit mass and $c = s\rho$ the specific heat per unit volume, the most general formula for the measure-ratio of heat is

$$Q = MS\Theta = L^3C\Theta. \tag{14.10c}$$

Hence we have:

quantity.	*formula.*	*measure-ratio.*
temp. excess of body (θ) .		Θ
heat-loss per unit area per unit time per degree (h) .	$h = q/At\theta$ . .	$H = Q/L^2T\Theta$
thermal conductivity of the fluid (k) . . .	$dq/dt = k \times \text{area} \times d\theta/dx$	$K = Q/LT\Theta$
$s\rho$. heat of fluid per unit *volume*	$c = q/\text{vol.} \times \theta$.	$C = Q/L^3\Theta$

We have here retained Q in the logometric formulae. But we could regard it as depending on our change of c by means of (14.10c):

$$H = LC/T, \quad K = L^2C/T. \tag{14.10d}$$

Consider forced convection, i.e. the cooling or the heating of a wire or pipe in a stream of fluid: a case which is of great practical importance, e.g. for radiators and air-cooled engines. In addition to h, k, c we have μ/ρ, v (velocity) and d (diameter). We easily see that we have three tautometric products:

$$\mathrm{S} \equiv hd/k, \; Q \equiv c\mu/k\rho, \; R \equiv \rho vd/\mu.$$

Hence from (14.10)

$$\mathrm{S} = \varphi\,(Q, R),$$

an equation first given by Lord Rayleigh.—*Nature* 95 (1915), 66.

For heat transfer in the case of a fluid in turbulent flow inside a clean circular pipe, Dittus and Boelter [32] have given the formula

$$S = AQ^m R^n,$$

where $A = 0.0225$, $m = 0.4$, $n = 0.8$. This has been successfully checked on many fluids—air and other gases, water, hydrocarbon oils, various organic fluids—with Q ranging from 0.73 to 95 and R varying between 2500 and 160,000.

This result, as well as similar relations which could be cited, has of course an intrinsic interest of its own as well as practical importance for heat-engineers. But it is brought forward here, in addition to the hydrodynamical formula (14.10a), mainly for its general significance in the treatment of physical quantities.

(1) We see how numerical data can be manipulated and co-ordinated by means of very general considerations. While physical theory can often give us some idea or picture of the processes at work, and sometimes it suggests a new constant (e.g. Planck's quantum), the final result must be an equation of the type of (14.10), at which we can in many cases arrive (in its general form) without investigating theory or even in the absence of any theory. Furthermore, the functional relation may be given by several different theories.

(2) We see the importance of tautometric products in the expression of experimental laws. A relation between such products summarises very diverse experiments and even enables us to predict new results. We can also realise the relative importance of the various factors. Strictly speaking, there is no meaning in saying that a velocity (v) or a length (r) is 'small,' for by varying our units we can make the number v or the number r as large as we please. It used to be thought that Poiseuille's law held only for 'slow' velocity and a 'narrow' (capillary) tube. We see now that it holds for $R = \rho vd/\mu$ small, i.e. less than about 2000. This is accurate language, for R is tautometric. Hence it is applicable to pipes of 'large' diameter at 'ordinary' speeds of flow, provided—as in the case of crude oil and molasses—the viscosity is relatively high.

(3) In the particular case of heat-transmission we see how it

[32] *Univ. Calif. Pub. in Eng.*, 2 (1930) 443. The equation holds for hydrocarbon oils only when $R > 7000$; for lesser values of R the curve of Morris & Whitman must be used.—Badger and McCabe, *Chemical Engineering*, 1936², p. 135. When the fluid is being *cooled*, we must take $m = 0.3$.

is of great practical importance *not* to equate to unity the measure-ratio of specific heat, even though we adopt this useful convention in relating the scientific and the British system of units. We are thus enabled to retain specific heat in the product $Q = \mu s/k$ and to allow for its influence in the results for different substances.

(4) We can also see that, in general, we have a right to expect that the coefficients in the tautometric function φ are numerically neither very large nor very small relatively to unity. For these coefficients are *operational factors*, i.e. they result from mathematical processes such as integration. In many ordinary cases, there are also concealed *shape-factors*, such as the ratio r/l which we ignored in our hydrodynamical example. Or there may be explicit shape-factors, such as a/b if we are dealing with a rectangular channel of sides a and b. In ordinary examples such ratios as a/b are usually moderate numbers.

It follows that if we assume a relation between certain quantities and if we find that very large or very small numbers connect the relevant tautometric products, it is likely that we are ignoring some other quantity which is also concerned in the process. Consider an example discussed by Einstein.[32a] The analysis of specific heat in liquids and solids presupposes a connection between the interatomic (or intermolecular) forces determining elasticity and those concerned in infra-red frequencies. That is

$$f(n, \beta, m, v_m) = 0,$$

where n is the characteristic frequency and β the compressibility; m is the atomic (or molecular) mass $= wm_{\mathrm{H}}$, where w is the atomic (or molecular) 'weight' and $m_{\mathrm{H}} = 1.665 \times 10^{-24}$ gram is the mass of a hydrogen atom; and $v_m = m\rho$ is the atomic volume. Now

$$N = 1/T \text{ and } [\beta] = LT^2/M.$$

Hence

$$N = L^{\frac{1}{2}}[\beta^{-\frac{1}{2}}]M^{-\frac{1}{2}}.$$

Therefore

$$\begin{aligned} n &= Cv_m^{\frac{1}{6}}\beta^{-\frac{1}{2}}m^{-\frac{1}{2}} \\ &= Cm_{\mathrm{H}}^{-\frac{1}{3}}w^{-\frac{1}{3}}\rho^{-\frac{1}{6}}\beta^{-\frac{1}{2}}, \end{aligned}$$

[32a] AP 35 (1911) 686. Debye subsequently gave a theoretical formula containing $7.4 \times 10^7 f(\sigma)$, where σ is Poisson's ratio (tautometric), instead of 3.3×10^7. —AP 39 (1912) 816. *Cf.* also E. Gapon, ZfP 44 (1927) 600.

where $m_{\mathrm{H}}^{-\frac{1}{3}} = 8.6 \times 10^7$. Experiment gives for solids

$$n = 3.3 \times 10^7 / w^{\frac{1}{3}} \rho^{\frac{1}{6}} \beta^{\frac{1}{2}}.$$

That is, $C = 0.38$, i.e. a moderate number. Taking w as molecular weight, we find that the formula also gives the greatest frequency of the infra-red absorption bands of a liquid.

4. Measure-Ratios in Electromagnetics.

Already in Chapter II we have given everything that a student wants to know concerning electrical and magnetic units. Compared to the usual apparatus of 'dimensions,' our account was extremely simple and elementary. Unfortunately we must now proceed to investigate and clarify a number of existing expositions. We propose in fact to apply common-sense principles to an unnecessary and bulky portion of the literature of electromagnetics.

Applying the elementary algebra of measure-ratios to a general change of units, we easily construct the following table :

Equation	*Logometric formula*
$f = qq'/\alpha r^2$	$Q = M^{\frac{1}{2}} L^{3/2} [\alpha^{\frac{1}{2}}]/T$
$Vq = \text{work}$	$[V] = M^{\frac{1}{2}} L^{\frac{1}{2}} / T [\alpha^{\frac{1}{2}}]$
$V = j\rho$	$R = T/L[\alpha]$
$f = mm'/\beta r^2$	$[m] = [\beta^{\frac{1}{2}}] M^{\frac{1}{2}} L^{3/2}/T$
$H = f/m$	$[H] = M^{\frac{1}{2}} / L^{\frac{1}{2}} T [\beta^{\frac{1}{2}}]$
$H = 2\pi j/ar$	$[a] = Q[\beta^{\frac{1}{2}}]/M^{\frac{1}{2}} L^{\frac{1}{2}}$

(14.11)

These results are obvious and the notation is the same as in Chapter II. Only now, instead of keeping to the c.g.s. units, we insert the measure-ratios L, M, T ; $[m] = m_1/m_2$ is employed for the measure-ratio of pole-strength to distinguish it from the mass-ratio M. The first thing to observe is that the charge-ratio Q depends not only on the arbitrary ratios L, M, T, but also on the arbitrary ratio $[\alpha^{\frac{1}{2}}] = \alpha_1^{\frac{1}{2}}/\alpha_2^{\frac{1}{2}}$. This last number can be varied without altering the units of length, mass and time. For example, if the suffix 1 refers to elst and 2 to elm measure : $L = M = T = 1$, $\alpha_1 = 1$, $\alpha_2 = c^{-2}$, so that $Q = [\alpha^{\frac{1}{2}}] = c$. This was what we did in Chapter II.

However, in formulae (14.11) we have decided—rather academically but really in order to disentangle the 'dimensions' of our text-books—to retain the symbols L, M, T to allow for possible

changes in the units of length, mass and time. Eliminating Q from the first and last equations, we find

$$[a/\alpha^{\frac{1}{2}}\beta^{\frac{1}{2}}] = L/T = U, \tag{14.12}$$

where U is used for the measure-ratio of velocity in order to distinguish it from V the potential or e.m.f. This is a generalisation of (2.48). Equation (14.12) is usually written in the form

$$1/[\varkappa^{\frac{1}{2}}\mu^{\frac{1}{2}}] = U. \tag{14.13}$$

This contains the arbitrary but quite legitimate assumption that $[a] = 1$, i.e. a_1 is taken to be equal to a_2, just as $a_1 = a_2 = 1$ for the elm-mag and elst-max systems. But it also contains the unjustifiable identification of α with $\varkappa$ and β with μ. This identification has already been adequately refuted; but the question will again be considered in the next chapter.

Having replaced the vague undefined idea of 'dimensions' by the simple substitution of measure-ratios, we can in a few lines dispose of the acrimonious dispute as to whether c is a velocity or a pure number. The obvious answer is that it is *both*. The problem whether c has dimensions is without ascertainable meaning. For, as already pointed out, no measure *has* dimensions in the sense of a measure-ratio; we cannot say that c 'has' c/c'. If we change our units of length and time, then $[c] = L/T$. If, as happens in practice, we decide to adhere to c.g.s. units, then the measure-ratio of c is unity; in current jargon, it is 'dimensionless.'

We are now in a position to deal with another serious misunderstanding. According to the text-books,[32b] 'it is unreasonable to suppose that one and the same quantity can have two different dimensions.' In so far as this statement has any meaning, it would be perfectly reasonable to assign two million different 'dimensions' to one and the same quantity. Consider the measure-ratio of charge

$$Q = L^{3/2}M^{\frac{1}{2}}T^{-1}[\alpha^{\frac{1}{2}}].$$

Every one of these four factors on the right-hand side is quite arbitrary; the measure-ratio Q can have any value we please to give it. Moreover, we must reject the contention that 'electrical units cannot have dimensions both on the electrostatic and the

[32b] Starling, *Electricity and Magnetism*, 1921, p. 390. 'That a single physical entity may possess more than one dimensional value is to the author unthinkable.' —Lanchester, p. 125.

electromagnetic systems.' [33] For it becomes meaningless to speak of ' dimensions ' in, on, or according to, the elm, elst or any other system of units. So long as we keep to any one system, our quantities have no dimensions at all; measure-ratios occur only when we change from one system of measurement to another. It follows that the tables of electrostatic and electromagnetic dimensions given in the text-books have, strictly speaking, no meaning at all; or, if interpreted as measure-ratios, they are discrepant and inconsistent. Take, for instance, two of the formulae given in (14.11):

$$Q = M^{\frac{1}{2}}L^{3/2}[\alpha^{\frac{1}{2}}]/T,$$
$$[m] = M^{\frac{1}{2}}L^{3/2}[\beta^{\frac{1}{2}}]/T.$$

What does Maxwell do? ' In the electrostatic system ' (ii. 266) he takes $[\alpha] = 1$ and $[\beta] = 1/U^2$, so that

$$Q = M^{\frac{1}{2}}L^{3/2}/T, \quad [m] = M^{\frac{1}{2}}L^{\frac{1}{2}}.$$

That is, he changes from

$$f = q_1q_2/r^2, \quad f = c^2m_1m_2/r^2$$

into

$$f' = q_1'q_2'/r'^2, \quad f' = c'^2m_1'm_2'/r'^2,$$

where $c/c' = L/T$. Starting with the elst-max system, he invents a new system of measures based on units different from the c.g.s. He has not explained how in doing so he has remained ' in ' the ' electrostatic ' system. On the other hand, ' in the electromagnetic system ' he takes $[\alpha] = 1/U^2$ and $[\beta] = 1$, so that

$$Q = M^{\frac{1}{2}}L^{\frac{1}{2}}, \quad [m] = M^{\frac{1}{2}}L^{3/2}/T.$$

He concludes that ' this system of units is not consistent with the former system ' (ii. 263). What he should have said is that, starting from *any* given system, he has adopted two discrepant changes of units. The fact that in the elst-max system $\alpha = 1$ and $\beta = 1/c^2$, does not at all involve $[\alpha] = 1$ and $[\beta] = 1/U^2$. We need not make this last decision; starting from the elst-max system, we can take any values for the new a', α' and β' compatible with (14.12), i.e. so that $a'^2/\alpha'\beta' = c'^2$. As a particular case, we could thus reach the elm-mag system! There is nothing compulsory, nothing ' electrostatic,' about Maxwell's assumption: $a' = \alpha' = 1$, $\beta' = 1/c'^2$. A similar remark applies to his ' electromagnetic ' dimensions. There is no difficulty whatever in

[33] N. Campbell, vii. 385.

making these dimensions the same, i.e. in taking the same measure-ratios no matter from which set of units we start. It all depends on what new system of units we want to devise ; and there does not seem to be any particular reason why we should devise any new system at all. All these dimensional formulae have been excogitated under the delusion that we are thereby privileged to obtain a glimpse into the 'nature' of things.[34] This incorrigible optimism still persists in electricians ; curiously enough, it is even more pronounced in practical men than in theorists.

5. Similar Systems.

The transformation $x_1 = Lx_2$, $m_1 = Mm_2$, $t_1 = Tt_2$, has been called logometric, for the constants L, M, T are measure-ratios. From this we at once deduced derivative measure-ratios such as $V = L/T$. We interpreted x_1, x_2 and similar pairs to mean two measures of the same magnitude in different units. But the transformation is susceptible of a second very useful interpretation. Instead of comparing two sets of measurements of the same system referred to different units, let us compare the measurements of two different systems referred to the same units. We shall call the systems S_1 and S_2 and we assume a one-to-one correspondence between them so that for every quantity q_1 in S_1 there exists a corresponding quantity q_2 in S_2. The corresponding quantities are either measures of conspecific magnitudes or identically defined compound measures. Capital letters, such as $Q = q_1/q_2$, will now denote the ratios of corresponding measures. Thus to every length x_1, x_1', . . . in S_1, there corresponds a length $x_2 = Lx_1$, $x_2' = Lx_1'$, . . . in S_2. Formerly L was the same for the measure-pairs of any Length ; that is, no matter what Length we chose for measurement relatively to the two *given* units, L was the same, being in fact the inverse ratio of these two units. So now L is taken to be the same for all corresponding length-pairs in the two given systems ; that is, no matter which pair of corresponding lengths we choose for comparison, their ratio is the *constant* L. In other words, our transformation is still a linear transformation with constant coefficients L, M, T.

[34] Thus we are told in the most recent book that 'in physics the term dimensions denotes the kind or quality of a physical entity.'—Lanchester, p. vii.

Two mechanical systems so related are said to be *dynamically similar*.

Let us analyse these conditions. $L =$ constant, implies geometrical similarity. If the lettered points (Fig. 73) designate corresponding points, we have $O_1A_1/O_2A_2 = O_1B_1/O_2B_2 = A_1B_1/A_2B_2 =$ etc. $= L$. The two systems represent geometrically the same figure on different scales, like two different-sized maps of the same district.

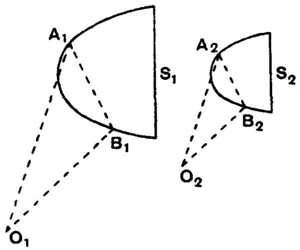

Fig. 73.

If we have both L and T constant, we have what may be called kinematic similarity. Suppose S_1 to be moving relatively to O_1 and S_2 relatively to O_2. If there is a constant factor [35] T such that $t_1 = Tt_2$, and if S_1 and O_1 at the instant t_1 are geometrically similar to S_2 and O_2 at the instant t_2, then we can say that the systems remain geometrically similar (as they were at the moment $t_1 = t_2 = 0$), but on different time-scales. That is, the systems have geometrically similar configurations at the instant ($t_1 = t_2 = 0$) from which duration is measured in each; and the configuration which S_1 has after the lapse of any interval t_1 is similar to that which S_2 has after the lapse of an interval $t_2 = t_1/T$, where T is a constant. The measure-ratio of velocity—that is, the ratio of the velocities of points in corresponding positions at corresponding moments—is

$$V = \frac{v_1}{v_2} = \frac{dx_1}{dt_1}\Big/\frac{dx_2}{dt_2} = \frac{dx_1}{dx_2}\Big/\frac{dt_1}{dt_2} = L/T.$$

It is therefore constant. Similarly the measure-ratio of acceleration is the constant $L/T.^2$

Suppose now that the three ratios L, M, T are constant. In addition to the foregoing conditions we have $m_1/m_2 = m_1'/m_2' =$ etc. $= M$. Then clearly the corresponding forces are in the constant ratio $F = ML/T^2$. All our previously tabulated logometric formulae are, with this new interpretation, applicable to dynamically similar systems. Formerly we took the measure-

[35] More generally: suppose that we can find two instants t_1' and t_2' and a constant factor T so that $t_1 - t_1' = T(t_2 - t_2')$. But we can put $t_1' = t_2' = 0$ without loss of generality. Durations, not dates, are involved.

ratio of a quantity to be the factor (Q) by which its measure (q_2) in one set of units must be multiplied to give its measure (q_1) in another set of units. We now take measure-ratio to be the factor (Q) by which the measure (q_2) of a quantity must be multiplied in order to give the measure (q_1) of the corresponding quantity in a dynamically similar system. Our formulae of the type $Q = L^x M^y T^z$ are still valid.[36] No new treatment is required.

In connection with changes of units we applied the term *tautometric* to those quantities or products whose measure is unaffected by a change of units, e.g. $\gamma m/c^2 r$, $\varkappa$, μ. We shall now apply the term *symmetric* to those quantities or products which have the same value (in any one set of units) in two similar systems. The terms are not always synonymous. While $\gamma m/c^2 r$ is both tautometric and symmetric, $\varkappa$ and μ, though necessarily tautometric, need not be symmetric ; we have $[\varkappa] = \varkappa_1/\varkappa_2$, where $\varkappa_1$ and $\varkappa_2$, the inductivities in the two systems, are not necessarily equal. So-called ' dimensional constants ' such as γ and c, while not tautometric, are symmetric ; for $[\gamma] = \gamma_1/\gamma_2 = 1$, since the gravitational constant is the same in all systems, provided, of course, that we keep to the same set of units.

Before applying this new interpretation of measure-ratio to electromagnetics, we shall illustrate this simple calculus of similarity by applying it to mechanics. Suppose the only forces occurring are elastic. If q is the modulus, $q =$ stress/strain. Hence

$$Q = F/L^2 = M/LT^2.$$

The velocity of an elastic wave (v) depends on q and the density (ρ). Since $V = L/T$ and the measure-ratio of density is $D = M/L^3$, we have $V = Q^{\frac{1}{2}}D^{-\frac{1}{2}}$ or $v =$ const. $(q/\rho)^{\frac{1}{2}}$. For similar systems (e.g. bells or tuning-forks) made of uniform isotropic material and vibrating with frequency n in virtue of elasticity, n depends on (l) the linear dimensions, q and ρ. Since $N = 1/T$, we have

$$n = \text{constant } q^{\frac{1}{2}}/\rho^{\frac{1}{2}}l.$$

Hence for vibrators of the same material, the pitch is inversely proportional to the linear dimensions. This law was stated by Savart in 1825 as the result of elaborate experiments.

[36] If we include the temperature, we have (14.10b) $Q = L^x M^y T^z \Theta^w$, where $\Theta = \theta_1/\theta_2$ is the measure-ratio of temperature-interval. Temperature is a fourth basic measure. Systems for which Θ as well as L, M, T, are constant may be called *physically* similar.

Another application is to similar systems in which the forces are due to gravity. Since g is the same for both

$$1 = G = L/T^2 = V^2/L.$$

This gives the formula const. $(l/g)^{\frac{1}{2}}$ for the period of a pendulum. If in addition there are forces proportional to area $\times$ speed^2, as is approximately true for ship-resistance,

$$F = V^2L^2 = L^3 = M = W,$$

if the density is the same ($M = L^3$). The resistance-forces are therefore in the same ratio as the gravity-forces (weights). Hence the two systems are dynamically similar. The rule $V^2 = L$ is known as Froude's law: corresponding velocities vary as the square root of the scale.

Let us now apply the transformation of similarity to Maxwell's macroscopic equations (in elst-mag units)

$$\text{curl } \mathbf{E} = -\mu/c \,.\, \dot{\mathbf{H}}, \quad \text{curl } \mathbf{H} = 4\pi\rho\mathbf{v}/c + \varkappa\dot{\mathbf{E}}/c.$$

Since $[c] = 1$, we have at once

$$[E]/L = [\mu H]/T,$$
$$[H]/L = Q/L^2T = [\varkappa E]/T.$$

Whence

$$1/[\varkappa^{\frac{1}{2}}\mu^{\frac{1}{2}}] = L/T = U. \tag{14.14}$$

Equation (14.14) may also be obtained without invoking Maxwell's equations. From equation (4.9)

$$\mathbf{A} = \beta\frac{j}{a}\int \mathbf{ds}/r$$

we deduce $[A] = Q\ /T$, since β and a are the same for the two systems. Equation (4.30), extended as in (5.13) to include magnetism, is

$$V = -\frac{\mu}{a}\frac{d}{dt}\int(\mathbf{A}\mathbf{ds}).$$

Whence

$$[V] = [\mu][A]L/T.$$

But the measure-ratio of e.m.f. or potential is, in a medium of constant inductivity,

$$[V] = [q/\varkappa r] = Q/[\varkappa]L.$$

From these relations we at once obtain (14.14).

It will be noticed that (14.14) is formally identical with the erroneous equation (14.13). We rejected this former equation

when 'dimensions' were interpreted, as they must be when discussing units, as the ratios of the measures of the same quantity when two different sets of units are employed. For in this case $[\varkappa] = [\mu] = 1$, since these quantities are independent of our units of measurement. But in the present case, $[\varkappa] = \varkappa_1/\varkappa_2$ and $[\mu] = \mu_1/\mu_2$ are not necessarily unity. We have therefore found that equation (14.13) can be validated when it is identified with (14.14), i.e. when we interpret 'dimensions' as the ratios of the measures (in the *same* units) of corresponding quantities in two similar systems.

It also follows that $v_1\sqrt{\varkappa_1\mu_1} = v_2\sqrt{\varkappa_2\mu_2} = c$, the value of v when $\varkappa = \mu = 1$; hence $c/\sqrt{\varkappa\mu}$ is a characteristic velocity of an electromagnetic system.[37]

The above was written under the impression that any employment of the formula $f = mm'/\beta' r^2$, where $\beta' = \beta\mu$, had been avoided. On reconsidering the argument, I believe that this formula has been tacitly assumed in Maxwell's macroscopic and similar equations. In other words, we are ignoring the existence of permanent magnets. The formulae we are really using are these :

$$F = Q^2/[\alpha']L^2{}_1$$
$$F = [m^2]/[\beta']L^2{}_1$$
$$F/[m] = [H] = Q/LT.$$

Whence

$$L/T = 1/[\alpha'^{\frac{1}{2}}\beta'^{\frac{1}{2}}] = 1/[\varkappa^{\frac{1}{2}}\,\mu^{\frac{1}{2}}],$$

since $[\alpha] = [\beta] = 1$.

Hence it is only by neglecting permanent magnets that we have established (14.14) for similar systems.

That this limitation is implied is made clear by considering the proof of the equation (14.10): $\varphi(p_1, p_2 \ldots) = 0$. The proof of this, which has been given above in an abbreviated non-

[37] Contrast the usual use of the erroneous equation (14.13). '$LT^{-1} = 1/\sqrt{\kappa\mu}$. Now LT^{-1} is a velocity v. Thus $1/\sqrt{\kappa\mu}$ must be a velocity. . . . LT^{-1} in units is in absolute c.g.s. units = 1 cm./sec.'—Loeb, p. 70*. Here there is no reference whatever to similar systems ; units, measures, and dimensions (whatever they are supposed to mean) are all confused. There is, of course, no truth in the legend that Maxwell arrived at the electromagnetic theory of light by elementary reasoning on 'dimensions.' 'Did not Maxwell himself arrive at the electromagnetic character of light by the purely mathematical analysis of the dimensions of the ratio between the electrostatic and electromagnetic unit ? '—Morris R. Cohen, *Reason and Nature*, 1931, p. 216.

mathematical form, depends entirely on the assumption that any general physical relationship is valid for all consistent systems of units. Hence the p's denote tautometric products. What we are now assuming is that the equation holds when the p's are symmetric products. This involves certain limitations.

(1) In the first place, each p is primarily tautometric; only by a secondary consideration can it be regarded as symmetric. Hence, when electric and magnetic quantities are involved, the products may contain α or β but cannot contain $\varkappa$ or μ except on one condition: that we are using the formulae for the apparent forces, with $\alpha' = \varkappa\alpha$ and $\beta' = \mu\beta$, investigated in Chapter II. And as we have seen, these are incompatible with the existence of permanent dipoles.

(2) Quantities which are really symmetric such as the universal constants c and γ, as well as constants which are so in practice—e.g. the acceleration (g) of gravity—will occur in these products. We often lose a great deal of obtainable information—as we shall presently see in the case of Tolman's transformation—if we prematurely equate these measure-ratios to unity (e.g. $C = \Gamma = 1$). In the case of specific heat we have already met the curious instance of a quantity which *de facto*, by a subsequently imposed convention, is tautometric. And we have seen that it is highly desirable to reject this convention when forming tautometric products. Applying the result to physically similar systems, we then have specific heat for different substances entering into the relevant symmetric products.

(3) In the case of similar systems we must remember that, while there are certain derived quantities, such as velocity, which are determined by the basic ratios (L, M, T), there are others which are not determined by L, M, T, but rather serve to determine these so that the systems may be similar. These quantities, which are dependent on external agencies or on complex constitutional factors, may be called the *characteristics* of the system. Consider a hydrodynamical system in which the force (f) on a body (one spatial dimension being l) depends on the velocity (v), viscosity (μ) and density (ρ), so that $F(f, v, l, \mu, \rho) = 0$. Forming symmetric products with ρ, v, l as probasic, we can express this in the form $\varphi(p_1, p_2) = 0$, where $p_1 = f/\rho v^2 l^2$ and $p_2 = \mu/\rho v l$. Here we have two characteristics ρ and μ, whose measure-ratios are $D = M/L^3$ and $E \equiv [\mu] = M/LT$. Now the ratios of density and viscosity are not in the least determined by

any arbitrary L, M, T we choose. Leaving temperature out of account, they are determined by the liquids we select for our systems. For two given liquids D and E are fixed and we must adjust our L, M, T accordingly; that is, our selection is limited by having to satisfy the relations: $M = DL^3$, $T = L^2D/E$, so that while L is arbitrary M and T are determined for any selected scale-ratio. If the phenomenon also depends on gravity (g), we have three characteristics g, μ, ρ, and the equation becomes $\varphi(p_1, p_2, p_3) = 0$, where $p_3 = gl/v^2$. Since $G = L/T^2$ (and in practice is unity), the ratios L, M, T are now uniquely determined:

$$L^3 = E^2/GD^2, \quad M = E^2/GD, \quad T^3 = E/G^2D.$$

Let us apply this to systems in which electric charge and inductivity are characteristics. Maxwell (i. 120) states that 'in similar systems the force is proportional to the square of the e.m.f. and to the inductive capacity of the dielectric but is independent of the actual dimensions of the system.' This is easily seen, for $f = qq'/\varkappa\alpha r^2$ gives $F = Q^2/[\varkappa]L^2$ since $[\alpha] = 1$, the units remaining the same. And, since $V = \Sigma q/\varkappa r$, $[V] = Q/[\varkappa]L$. Hence $F = [V^2\varkappa]$, or f varies as $V^2\varkappa$.

Similarly we can show that a system of electric charges subjected only to electrostatic forces is never in equilibrium. For the energy $w = \Sigma qV$ (with $\varkappa = 1$) gives $W = Q^2/L$. If we take $Q = 1$ and $L > 1$, we have a possible deformation of the system. Since $W = 1/L$, the energy decreases; hence the equilibrium was not stable. It easily follows from this that a sphere is a figure of unstable equilibrium, we must add other forces besides the electrostatic.

In electric-magnetic similar systems the characteristics may be taken to be q, $\varkappa$ and μ. We have

$$K \equiv [\varkappa] = Q^2T^2/L^3M,$$
$$[\mu] = L/TK^{\frac{1}{2}} = LM/Q^2.$$

Instead of the latter equation we can take

$$C = 1/[\varkappa\mu]^{\frac{1}{2}} = L/T.$$

Let us find the condition that any other measure $R = L^xM^yT^zQ^w$ can be expressed as C^aK^b. Inserting the values of C and K, equating indices and eliminating a and b, we find

$$x - y + z = 2y + w = 0.$$

Hence

$$R = L^xM^yT^{y-x}Q^{-2y},$$

where x and y are arbitrary. We can now investigate the possibility of a relation between two quantities, say f and g, by seeing whether one of the R's can be found so that $F = RG^n$.

For example, let u be the energy-density so that $U = M/LT^2$ and let E be the electric field-intensity so that $[E] = LM/T^2Q$. Then if u is a function of E, let $U = R[E^n]$ or

$$L^{-1}MT^{-2} = L^xM^yT^{y-x}Q^{-2y}(LMT^{-2}Q^{-1})^n.$$

Equating the indices, we find $n = 2$ and $R = K$. That is, $u =$ constant $\times\ \varkappa E^2$. Similarly, if q is the charge of a complex, f its acceleration and w its rate of emission of energy ($W = L^2MT^{-3}$), we can show that $w =$ const. q^2f^2.

For the case of empty space (or the same medium in both systems) we have $C = K = 1$. Hence the transformation can be expressed as

$$L = \lambda, \quad T = \lambda, \quad Q^2/M = \lambda. \qquad (14.15)$$

As a particular case we could take $Q = 1$, $\mathrm{M} = 1/\lambda$.

Let us next consider the phenomena of electromagnetic radiation which are assumed to have the three characteristics :

$$\begin{aligned}&\text{light-velocity } c: \ \mathrm{C} = L/T\\ &\text{Boltzmann's gas-constant } k: \ K = L^2M/T^2\Theta\\ &\text{Planck's quantum constant } h: \ H = L^2M/T. \qquad (14.15\text{a})\end{aligned}$$

The condition that a fourth quantity p, with measure-ratio $P = L^xM^yT^z\Theta^w$, should be a physical constant, i.e. that p should form a symmetric product with c, k, h, is identical with the condition that four derived quantities depending on four basic should form a tautometric product. From a result previously given (14.9) for three quantities it is easily seen that the condition is

$$\begin{vmatrix} x & y & z & w \\ 1 & 0 & -1 & 0 \\ 2 & 1 & -2 & -1 \\ 2 & 1 & -1 & 0 \end{vmatrix} = 0$$

or

$$x - y + z - w = 0.$$

Hence the measure-ratio of the typical secondary characteristic is

$$P = L^xM^yT^z\Theta^{x-y+z}.$$

Let us apply this to finding the possibility of a law connecting u, the specific density of black radiation in vacuum ($U = L^{-1}MT^{-2}$), and the absolute temperature θ. Take $U = P\Theta^n$. We easily

find $n = 4$, so that $u = a\theta^4$. This is the well-known Stefan-Boltzmann law. Similarly we find that if p is the pressure of black radiation in a cavity of volume v, $p =$ const. $v^{-4/3}$. Suppose we wish to investigate the possibility of a relation between energy (E) and frequency (n). We have $[E] = ML^2/T^2$ and $N = 1/T$. Taking $W = PN^a$, we find

$$[E] = L^2MT^{-1}N = HN,$$

or E is proportional to hn.

We can see these results more clearly if we use symmetric products or, better still, tautometric products—since C, H, K are unity for similar systems but are not unity for a change of units. Consider the specific density of black radiation in vacuum (u), and also the density ($w = du/dn$) of radiation of a given frequency, at the absolute temperature θ. We have the logometric formula

$$U = L^{-1}MT^{-2} = C^{-3}(K\Theta)^4H^{-3},$$

so that $uc^3h^3/k^4\theta^4$ is tautometric. Hence if u is assumed to depend only on the constants c, h, k and the temperature θ,

$$u = A(k^4c^{-3}h^{-3})\theta^4,$$

where A is an absolute or monometric constant, which we know *aliunde* (from Planck's theory) to be $4\pi^5/45$. We also have $W = M/LT$ and $N = 1/T$, so that

$$N = T^{-1} = K\Theta H^{-1},$$
$$WN^{-3} = L^{-1}MT^2 = C^{-3}H.$$

That is, wc^3/hn^3 and $k\theta/hn$ are tautometric. If then we assume

$$w = f(c, h, k, n, \theta),$$

we infer from (14.10) that

$$wc^3/hn^3 = \varphi(hn/k\theta). \qquad (14.15b)$$

Now wdn is the energy between the frequencies n and $n - dn$, and if $u_\lambda d\lambda$ is that between the wave-lengths λ and $\lambda + d\lambda$,

$$u_\lambda d\lambda = -\,wdn = wcd\lambda/\lambda^2,$$

since $n = c/\lambda$ and $dn = -\,cd\lambda/\lambda^2$. That is, $u_\lambda = wc/\lambda^2$. Hence

$$u_\lambda = ch\lambda^{-5}\varphi(ch/\lambda k\theta).$$

And we know from Planck's work that

$$\varphi(x) = 8\pi[\varepsilon^x - 1]^{-1}.$$

Thus we see that from elementary logometric considerations we can arrive at the general form of Planck's equation.

It is easy to see that we can express the transformation as follows (λ now standing for L) :

$$L = \lambda,\ T = \lambda/C,\ M = H/C\lambda, \Theta = HC/K\lambda. \quad (14.15c)$$

To which we can add the charge-ratio

$$Q^2 = ML^3/T^2 = HC.$$

Thus for energy and frequency

$$[E] = ML^2/T^2 = HC/\lambda,$$
$$N = 1/T = C/\lambda,$$

or

$$[E] = HN, \text{ i.e. } E = hn.$$

Similarly

$$W = M/LT = H/\lambda^3,$$
$$W/N^3 = H/C^3,$$
$$N/\Theta = K/H.$$

Whence, under the previous assumption, we infer (14.15b) as before.

If we apply (14.15c) to similar systems we can put $C = H = K = 1$.

Hence the transformation of similarity for systems of electromagnetic radiation is

$$L = \lambda, \quad M = 1/\lambda, \quad T = \lambda, \quad \Theta = 1/\lambda. \quad (14.16)$$

If we combine this with the transformation (14.15) for electromagnetic systems, we have only to add the charge-ratio

$$Q = 1.$$

It follows from (14.16) that any connected quantity p must have its measure-ratio expressible in the form $P = \lambda^a$. Similarly for another quantity q, $Q = \lambda^b$. If then we have $p = f(q)$, this equation becomes for a similar system

$$Pp = f(Qq)$$

or

$$p = \lambda^{-a} f(q\lambda^b)$$

for all values of λ. Clearly the solution of this functional equation is

$$p = Aq^{a/b},$$

where A is a constant. Suppose for example we wish to investigate the possibility of a relation between energy (E) and frequency

(n). Since $[E] = ML^2/T^2 = 1/\lambda$ and $N = 1/T = 1/\lambda$, we have $E = An$. According to Tolman,[38] 'by this simple process we have thus derived the fundamental equation of the quantum theory.' But surely it would be ridiculous to claim that we have proved or derived the equation $E = hn$. All we have shown is that *if* there be a relation between E and n, it must be of the form $E = An$. And we have not even proved that A is an absolute (monometric) constant; it might be a symmetric constant such as $\varphi(\theta/n)$.

The transformation (14.16) is what Tolman has called 'the principle of similitude.' 'If,' he says,[39] 'we should try to regard the principle of similitude as determining a system of dimensions, we should be obliged to say . . . that force, for example, has the dimensions $[l^{-2}]$.' In the same way presumably we should be obliged to say that Froude's law ($G = L/T^2 = 1$ and therefore $V^2 = L$) proves that time has the dimensions $[l^{\frac{1}{2}}]$! We have here another argument for getting rid of the word 'dimensions' with its absurd implications. Interpreting the symbols as measure-ratios, there is nothing peculiar about the equation $F = L^{-2}$, whether applied to changes of units or to similar systems.

Since this transformation is devised for certain characteristics, we can also see the absurdity of Tolman's attempt to extend it to all possible similar systems. For example,[40] he applies it to prove $v = f(q/\rho)$, where v is the velocity of a compressional wave in a liquid, q is the modulus of elasticity and ρ the density. Now since the transformation for similar elastic systems is specified by $F = QL^2$ (since stress $= q \times$ strain) while the present transformation gives $F = 1/L^2$, they are clearly irreconcilable. He also[41] tried to apply it to the Newtonian law of gravitation. But $f = \gamma mm'/r^2$ gives $[\gamma] = \lambda^2$, whereas of course $[\gamma] = 1$. The simple fact is that $[\gamma] = L/MT^2$ is not a characteristic for the present transformation. But rather than admit its restricted validity, Tolman attempted to restate the law of gravitation—he might as well try to reformulate Froude's law ($V^2 = L$) in order to reconcile it with his own transformation ($V = 1$).

Having thus classified Tolman's alleged principle of similitude as a similarity-transformation applicable to systems with certain characteristics and consequently inapplicable to others, we need

[38] *Theory of the Relativity of Motion*, 1917, p. 232.

[39] PR 8 (1916) 9. I. Maizlish gives it the even more pompous title of 'the principle of projective invariance.'—*Ibid.* 18 (1921) 1.

[40] PR 8 (1916) 233.

[41] PR 3 (1914) 253, 8 (1916) 11.

not trouble to refute the extravagant claims that have been made on its behalf.

> The fundamental entities of which the physical universe is constructed are of such a nature that from them a miniature universe could be constructed exactly similar in every respect to the present universe.—Tolman, PR 3 (1914) 244, 4 (1914) 145, 6 (1915) 224.
>
> If this hypothesis should turn out to be correct, it would carry with it implications so far-reaching that it might well affect the entire future of physical research.—Bridgman, PR 8 (1916) 423.

As an able critic has remarked,

> if such arguments can be published in a serious scientific journal and gravely discussed by professors of physics, it is surely proof that the nature of the argument from dimensions or physical similarity is not properly understood.—N. Campbell, vii. 419.

But we have here done more than adopt the cynical and negative attitude of Dr. Campbell. We have deduced the transformation from elementary principles and we have shorn it of its pretended far-reaching philosophical or scientific implications. Moreover we have, by our second interpretation or application of measure-ratios, cleared up the mysterious connection between 'dimensions' and similarity, which is so puzzling to students—and perhaps to their teachers.

6. 'Space-Time.'

There is at present in vogue a very abstruse and speculative hypothesis designated by the curious compound term 'space-time.' Our interest in it is twofold: it is based on what must be regarded as a serious misinterpretation of the symbols of physics, if the views already expounded in this chapter are correct; and it enters into the application of Einstein's theory to electromagnetics. In the present chapter we are not concerned with the latter aspect. For our present purpose it is quite unnecessary to understand the details of Einstein's theory. It is sufficient to apply some elementary metrological considerations to the 'interval' s, where $s^2 = r^2 - c^2t^2$. And we may even take this so-called interval to be zero, so that $r^2 = c^2t^2$. For this holds in all the alleged applications of 'relativity' to electromagnetics and optics, except one; and this one exception (concerning Fresnel's coefficient) is—as we have already hinted in Chapter IX—capable of a much simpler explanation.

In view of our previous discussion we can interpret the r as length (not position) and the t as duration (not date). We should therefore call s (which is zero) the length-duration of a transmission-process rather than space-time in the abstract. In the next place, we can take the expression as referring to subrelative systems. For, as Dr. Campbell observes (iv. 3), 'it is difficult to think of a single instance' in which the same phenomenon has been scientifically observed at the same time by an observer on earth and by an observer in a train, ship or airplane.

But, as he further says, 'the same observer can observe the same system first when it is at rest and then when it is in motion relative to him.' So we can get rid of the purely imaginary second observer and reduce Einstein's assertion to what is, in principle at any rate, verifiable by the only scientific court of appeal, the man in the laboratory. The theory can then be expressed thus: s^2 is the same for *SR* at rest and for *SR* moving uniformly with respect to the laboratory. Whether or how far this is true, will not be investigated here; given the medium (aether) which Einstein assumes, there is nothing intrinsically paradoxical or impossible about the theory.

We assume then that this theory, which except in very accurate measurements introduces quite negligible corrections, is to be proved or disproved in a scientific laboratory. The velocity c has been measured many times, so in principle the theory must be tested by measuring Length and Time. As we have already pointed out, these concepts are and must be accepted in their ordinary connotation by the fundamental measurers, i.e. the instrument-maker and the practical physicist. Hence the theory involves no new concepts; like all other theories, it is concerned with pure numbers, i.e. with the measures r, c, t; the theory can be examiued only *after* these measures have been obtained.

It is with a shock that we turn from these simple but fundamental considerations to the paradoxical pronouncements of the exponents of the theory. Listen to this from Sir James Jeans:

> Time and space as separate entities—the time and space we wrote about and thought about previous to 1905—have gone; or, as Minkowski puts it, have become shadows; while only the product of the two remains as the framework in which all material phenomena take place. Time and space separately may mean something to us subjectively; but Nature knows nothing of them until they

have been multiplied together into a four-dimensional space-time continuum. . . . Einstein's theory eliminates the supposed essential difference between space and time; what is one man's space is another man's time; not only so, but what is the past in time for one man is the future for another man. . . . Existence becomes a picture rather than a drama and the year 1927 has the same sort of existence as the county of Cornwall.—*Nature*, 117 (1926) 309 f.

Time and Space without essential difference, merely subjective until 'multiplied together'! What an extraordinary discrepancy between this popular outburst, which after all is merely typical, and the commonplace pedestrian treatment we have just outlined. Let us see if we can make anything of it. In 1907 Minkowski wrote:

I will employ complex quantities in a way not hitherto usual in physical investigations, operating with $t\sqrt{-1}$ instead of t. . . . Thereby, as I expressly emphasise, there is always question only of a clearer treatment of entirely real relations.—*Math. Ann.* 68 (1910) 475.

Putting $ict = l$, we have $s^2 = x^2 + y^2 + z^2 + l^2$, which enables us to employ 'four-dimensional geometry' to this manifold of numbers. This perfectly legitimate analytical device enables us to express the theory more succintly and neatly, using metaphors derived from Cartesian geometry. Flushed with enthusiasm for his expedient, Minkowski [42] declared: 'Henceforth space by itself and time by itself are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality.' And he made this pronouncement, 'safe in the twentieth century, without loss of reputation or suspicion of lunacy, and indeed with the heightened respect and regard of his peers.' [43] Which does not speak well for the critical faculty of contemporary physicists, in most of whom the itch for paradox seems to have outrun their common sense. Apparently it requires a great deal of moral courage to tell a big man in physics that, in vulgar parlance, he is talking through his hat.

In the first place, however we interpret $s^2 = r^2 - c^2t^2$, it is clear at any rate that all the constituents of the expression are measure-numbers. It is nothing but an algebraic quantity. It contains neither space nor time in the ontological sense, i.e. neither Length nor Duration. So it cannot possibly throw any

[42] In *The Principle of Relativity*, 1923, p. 75.

[43] Heyl, *New Frontiers of Physics*, 1930, p. 99.

light on these entities which do not occur in it. If we wish to inquire further, to get behind the measures, we must obviously examine the process of measurement. That is, we must leave the theorist and interrogate the man in the laboratory. The theory of relativity has certainly dealt with numbers represented by algebraic symbols. But has it introduced any change in our methods of measurement? Any practical physicist will at once answer: Not an iota. Whatever meaning Duration and Length had before the advent of 'relativity,' they have the same meaning now. Whatever methods we had for estimating length = Length/Unit Length are quite unaltered. Perhaps 'time,' i.e. Duration, was a 'mere shadow' in 1908; but if so, it was just as shadowy in 1808. The idea that certain combinations of measure-numbers have anything to tell us about philosophy or experience, is a sheer delusion based on ignorance of the meaning of scientific symbols.

Accordingly we reject as *ultra vires* the unjustified attempt of physical theorists to cajole the public into believing that somehow, by mathematical operations on paper, they have not only solved philosophical riddles, but dissolved the pragmatic beliefs of common sense. Typical of such claims are the following citations:

> The restricted physical theory of relativity introduced a revolution into the foundations of scientific thought by destroying the objectivity of time and space.—Jeans, *Enc. Brit.* 19 (1929[14]) 96.
>
> [Minkowski's] amazing discovery in 1908 that time and space are not separate things but constituent elements in the deeper synthesis of space-time.—General Smuts, *Nature* 128 (1931) 522.
>
> The theory of relativity, by merging time into space-time, has damaged the traditional notion of substance more than all the arguments of the philosophers.—Bertrand (Earl) Russell,[44] Introduction to Lange's *History of Materialism*, 1925, p. 11.

Our next objection is based on the even more elementary fact that the numbers involved (r and ct) are exclusively measures of *length*. When Einstein says,[45] 'we shall introduce the light-time $l = ct$ in place of the time t,' he means light-distance; just as light-year is a rather inappropriate name for a certain number of kilometres. We can, of course, draw a distance-time curve, the

[44] Contrast Russell's earlier view: 'The independence of space and time cannot in fact be contested without the grossest absurdities.'—*Rev. Mét. et Mor.* 6 (1898) 773.

[45] *The Meaning of Relativity*, 1921, p. 34.

numbers x and t being correlated according to definite scales with the perpendicular length-numbers on the paper. Lagrange [46] even said that ' we can regard mechanics as four-dimensional geometry and analytical mechanics as an extension of geometrical analysis.' So, according to Jeans,[47] ' the running schedule of the Cornish Riviera express,' i.e. the x-t graph, is ' a two-dimensional space obtained by welding together one dimension of ordinary space, namely length, and one dimension of time.' If this analytical manipulation of measure-numbers is all that is meant by all this talk of space-time, it is rather innocuous.

As Dr. N. Campbell says (iv. 60 f.) :

> ' Plotting ' is simply the substitution of one pair of relations for another pair, the numerical relation being the same in both pairs but the physical relation different. The numerical relation between (e.g.) the pressure and temperature of our gas is the same as that between the ordinates and abscissae of the point in the diagram by which we represent that pressure and temperature in our diagram. But the physical relation is utterly different ; in one case it is a relation between two properties of a perfect gas, in the other between two perfectly different properties of a sheet of paper ; chalk and cheese are not more different. . . . It appears to us paradoxical when the mathematician announces that space and time are merely different aspects of the same thing ; for we know that measuring times is perfectly different from measuring spaces. But his assertion is an immediate and almost inevitable outcome of our practice of representing every kind of magnitude—pressure or volume, mass or temperature—as a kind of space for the special purposes served by plotting.

In view of our previous discussion, we can express this more clearly. The ' physical relation ' between pressure and temperature, as the phrase is used by Campbell, means the objective or ontological connection of events which a man observes when experimenting in a laboratory. This, of course, is entirely different from, say, the spatial relations (with reference to perpendicular axes) of a curve drawn on paper. But, once more we point out, physics—apart from the number-producing operations of the laboratory—is exclusively concerned with pure numbers. The *equation* $f(p, \theta) = 0$ is *identical* with the equation $f(x, y) = 0$; p and θ, x and y, are pairs of numbers differently designated. Similarly the equation $\varphi(x, y, z, ct) = 0$ is simply a relation between ordinary algebraic quantities.

[46] *Œuvres*, 9 (1881) 338. [47] *The Mysterious Universe*, 1930, p. 100.

But apparently much more than this is meant :

> When we weld together length and breadth, we get an area—let us say a cricket-field. . . . If we further weld together an area (such as a cricket-field) of two dimensions and height (of one dimension), we obtain a space of three dimensions. . . . It is harder to pass from three to four because we have no direct experience of a four-dimensional space. And the four-dimensional space which we particularly want to discuss is peculiarly difficult to imagine, because one of its dimensions does not consist of ordinary space at all but of time. To understand the theory of relativity, we are called on to imagine a four-dimensional space in which three dimensions of ordinary space are welded to one dimension of time.—Jeans, *Mysterious Universe*, 1930, p. 98 f.

' Welding ' is rather a handy metaphor for bridging an awkward gap. We have already commented on the alleged evolution of a plane from length and breadth, and on the futility of trying to imagine four-dimensional Space as distinct from an analytical fourfold of numbers. This confusion of spatial and temporal magnitudes with numbers may be called a metaphysical blunder. But the following quotations exhibit the more elementary mistake of treating the ratio of length-measure to time-measure as if it were independent of the time-unit chosen, as if the measures r and t were comparable :

> The interval between a man's birth and death may be estimated by S as 1000 miles and 75 years, but S' may call it millions of miles and 76 years.—Jeans, PRS 97A (1920) 68.
>
> His time dimension extends over some $5 \cdot 10^{19}$ cm., whereas his spatial dimensions are less than 200 cm. along his longest axis.—Prof. F. A. Lindemann, in *The Mind*, ed. R. McDowall, 1927, p. 44.
>
> The change in the system of space and time for observers in relative motion . . . means a real rotation of the time-direction in the four-dimensional world ; a bit of space goes into time and a bit of time into space.—Eddington, *Monthly Notices*, 80 (1919) 98.
>
> When we choose the relation between the units of length and time so that the velocity of light is equal to unity, . . . the numerical measure, however we choose the single units, of the duration of our life will in any case be many millions of times greater than that of the spatial extent of our activity.—Thirring, *The Ideas of Einstein's Theory*, 1922², p. 78.
>
> Thus 2 yards to the right, 3 yards forward, 4 yards upward, and 5 ' yards ' later—a ' yard ' of time is to be interpreted as the time taken by light to travel a yard— . . . amounts to a displacement of two yards. When, as here, we consider displacement in time as well as in space, the resultant amount is called the *interval*.—Eddington, *New Pathways in Science*, 1935, p. 275.

What is one man's space is another man's time, and vice versa.—Jeans, *Atomicity and Quanta*, 1926, p. 8.

The passage of sunlight from sun to earth now reduces to nothing more than the continuity of a corrugated crumpling along a line in the continuum which extends over about 8 minutes of time and about 92,500,000 miles of length.—Jeans, *Mysterious Universe*, 1930, p. 113.

As for the fourth dimension, or time, it takes on a strange aspect. While the other three dimensions of things are short and almost motionless, it appears as ceaselessly extending and very long.—A. Carrel, *Man the Unknown*, 1936, p. 155.

Now when we declare these statements to be preposterous, it must be clearly understood that we are *not* arguing against the scientific hypothesis known as the special theory of relativity. That hypothesis may or may not be true; nevertheless the preceding assertions must be labelled absurd, in spite of the authority of the writers. Any comparison between feet and seconds is devoid of physical significance, the comparison would be entirely changed by measuring in miles and minutes. As a mere bit of algebra, we can of course compare the two numbers, distance-measure and time-measure; in the same sense as we might compare acres and dynes, volume and velocity. But this mere algebraic comparison does not occur in physics, nor is it required in 'relativity.' With all due respect to Sir Arthur Eddington, he makes an elementary blunder when he says his yard of time is to be interpreted as the time taken by light to travel a yard, and proceeds to treat it as vectorially combinable with a space-vector. We have to do with r yards and ct *yards*, where t is seconds and c is yards per second; not with r yards and $1/c$ the time taken by light to travel a yard. There is nothing in any scientific theory which asks us to 'add' 1000 miles and 75 years $(l + t)$, to say that a duration is many million times greater than a spatial extent $(t > l)$, or to assert that a time 'extends' over so many centimetres $(t = l)$.

We must distinguish (1) the objective entities and relations which exist prior to measurement; (2) the operations which originate measure-numbers, the ratio of two Lengths giving length or the ratio of two Times giving time; (3) the resultant 'physical quantities' which are numbers. To which of these is Jeans referring when he speaks of a man's life-interval being estimated in l miles $+\ t$ years, when he says that one man's time is another man's space? Not (1) or (2), for he cannot be

maintaining that what is Length to A is Time to B. Nor can it be (3), for all that ' relativity ' says is that when $r = ct$ for A, $r' = ct'$ for B.

Next we must point out, in continuation of our remarks in Chapter IX, that the use of ' four dimensions ' is a purely analytical expedient, a re-grouping of algebraic operations. It is supposed to have certain psychological advantages inasmuch as the spatial metaphors employed serve as a mnemonic ; sometimes this method is neater from the standpoint of the pure mathematician. But in general it exercises a fatal fascination on two classes of people : on the technical specialist who lacks a broad cultural education, and on the ordinary reader who accepts with childish credulity whatever any scientific bigwig chooses to print. Sometimes the result may be best described as mental auto-intoxication superinduced by an overdose of metaphors. ' What,' asks Lanchester (p. 116), ' would be the interpretation, as a matter of observation, in the event of such a thing as rotation or circulation about a time-axis being possible and taking place ? '

Chief among the cults of modern pseudo-physics is the worship of the root of minus one.

> The existence of the imaginary quantity $\sqrt{-1}$ expresses the fundamental difference between space and time.—Barnes, *Scientific Theory and Religion*, 1933, p. 116.
>
> This difference in sign may be regarded as reflecting the difference in the nature of spatial and temporal extension.—Tolman, *Relativity*, 1934, p. 31.
>
> It is known that time can be converted into space by multiplying by the factor ic.—Sir O. Lodge, PM 15 (1933) 708.
>
> The i . . . limits the physical equivalence of space and time.—Frenkel, *Wave Mechanics*, 1932, p. 8.

The i-devotees are much too modest. Can we not turn a hyperbola $x^2/a^2 - y^2/b^2 = 1$ into an ellipse $x^2/a^2 + y^2/c^2 = 1$, by taking $c = ib$? And why continue saying that for sound-waves $r^2 - c^2t^2 = 0$, when we can express the equation as $x^2 + y^2 + z^2 + l^2 = 0$ and say that the sound-source and the sound-receiver are separated by a zero four-dimensional distance ?

It is surely a bizarre idea that any satisfactory explanation can be derived from the introduction of the operator i, that it can in any way influence reality, that the algebraic device of four dimensions can produce a new physics. It has been well criticised by Dr. Campbell who, though a relativist, is also a laboratory worker (iv. 67) :

To explain is to interpret in terms of more acceptable ideas, and the ideas of Minkowski are essentially unacceptable. It does not help us to understand why all observers at rest relative to a system make the same observations or why observers in relative motion may differ as to the time sequence of events, if we are told that all our observations and all our measurements are nothing but the placing of points in a four-dimensional and partly imaginary time-space. For, in so far as we can understand such a statement, we know it to be false; time and space are not the same thing, neither is imaginary, and there are observations which are concerned with things that are neither time nor space. No facts are incomprehensible, though they may be surprising; an 'explanation' which is incomprehensible is no explanation at all.

7. 'Relativist' Units.

The idea that time can be converted into space, while it has been largely propagated by enthusiastic popularisers of Einstein's theory, is really based on a much more elementary expedient, namely, the choice of such units as make light-velocity unity. Retaining the cm. as unit of length, let us choose as time-unit the 'cosec' $= a$ seconds. The vel. of light in cm./cosec. is $c' = ac$, so that $a = 1/c = 1/(3 \times 10^{10})$ if $c' = 1$. We then have $s^2 = r^2 - t'^2$, where $t' = ct$ is in cosecs. Thus even in this case Duration is not measured in cms. (or kms.) but in cosecs, notwithstanding the following assertions.

The velocity of light being unity, a kilometre is also a unit of time $= (1/300{,}000)$ sec.—Eddington, *Report on the Relativity Theory of Gravitation*, 1920², p. 16.

If the unit of length be 1 cm., the unit of time employed is $1/c$ seconds, where $c = 3 \,.\, 10^{10}$. In the theory of relativity there is no absolute distinction between space and time, and so we refer to our time-unit as one centimetre, 1 cm. being the distance traversed by light in one time-unit.—F. Murnaghan, *Vector Analysis and the Theory of Relativity*, 1922, p. 114.

Thus the velocity of light is 1 and the dimensions of time become the same as those of length. The character of a primary magnitude ordinarily assigned to the time thus disappears, the unit of time being linked up with the unit of length by means of the phenomenon of the propagation of light.—Levi-Civita, *Absolute Differential Calculus*, 1927, p. 307.

The distances in time of two events are generally greater than their distances in space—let us not forget that 1 second of time equals 300,000 kilometres.—Borel, *Space and Time*, 1926, p. 163.

If provisionally we retain the centimetre as the unit of length, the unit of time must be made $2 \cdot 999 \times 10^{10}$ times as small as the second.

Here, again, making the constant unity requires us for convenience to make it dimensionless; and we therefore call the new unit of time the centimetre, and we write 1 sec. $= 2{\cdot}999 \times 10^{10}$ cm.—G. N. Lewis, PM 45 (1923) 269.

When we speak of units we mean magnitudes not quantities. So, even though we take $c' = 1$, it is nonsense to say that we measure. Time in cm. or km. The time-measure may be numerically equal to the length-measure, i.e. light traverses unit Length in unit Time. Nevertheless what we are measuring is not unit Length but the Time taken by light in traversing unit Length; we measure it in cosecs, not in cms.

As for the 'dimensions,' the measure-ratio of length is $L = 1$, that of time is $T = 1/c$. Hence L is not equal to T, and V is not unity but c. The common assertion [48] that, if we make $c' = 1$, velocity has 'zero dimensions,' is therefore incorrect. The statement is really one of those vague pronouncements made by people who fancy that 'dimensions' are a qualitative factor expressing the 'nature' of things. While the number $3 . 10^{10}$ is supposed capable of being associated with dimensions, for some reason or other the number 1 is supposed to be incapable.

The whole theory of the dimensionality of physical quantities has been obscured by the existence of physical constants, to which dimensions are often assigned. Now while it might seem reasonable to assign dimensions to R or k, it would be highly inconvenient to give dimensions to a pure number and especially to unity which does not even appear in the physical equations.—G. N. Lewis, PM 45 (1923) 268.

It would be difficult to state what exactly is the theory here referred to; nor is it apparent what connection it has with physics. As we have shown, velocity is always a pure number, whether it is 1 or $3 . 10^{10}$; and 'dimensions' can never be assigned or given to it. The issue is childishly elementary: if $c' = 1$, then $V = c/c'$ is c. The idea that by contemplating or manipulating such platitudes we can reach an insight into the nature of time and space, is an irrational delusion from which physicists—and especially relativists—should immediately rid themselves.

Sir James Jeans makes an ingenious attempt to justify confusion in physics by appealing to international trade.

[Space and time], although fundamentally similar in a way not yet fully understood, are measured in terms of different units.

[48] *E.g.*, Lewis and Adams, PR 3 (1914) 95.

There is a 'rate of exchange' between these two units, just as there is between Italian lire and pounds sterling; and this rate of exchange, the ratio of a unit of length to a unit of time, is measured by the velocity of light which we denote by c. If we wish to regard a second of time as a length, we treat it as being equivalent to three hundred million metres, this being the distance travelled by a ray of light in one second. In the same way there is a rate of exchange between . . . matter and energy; and this is known to be the square of the velocity of light, c^2.—Jeans, *Atomicity and Quanta*, 1926, p. 10.

What is 'the ratio of a unit of length to a unit of time'? If it is not the ratio of 1 to 1, it is a monstrosity; it certainly is not the number c. We may 'wish to regard a second of time as a length,' we may even wish to regard it as a colour; but we do not thereby achieve sense. If we assert that we treat 1 second as 'being equivalent to' $3 \,.\, 10^8$ metres, we are either inventing a new and unexplained use of the term 'equivalent' or else we are saying nothing more or less than this: the velocity of light is $3 \,.\, 10^8$ metres per second. Then why not say it? And why borrow a financial metaphor to describe velocity as the 'rate of exchange' between Length and Time? We cannot have a ratio or a rate except we *first* measure these magnitudes and express them as numbers.

The equation $r = ct$, where c may be *any* given velocity, may be expressed as $r = t'$, which we may restate more ambiguously as r km. $= t'$ cosec. As a particular case we have $3 \,.\, 10^5$ km. $= t'$ cosec.; since $t' = 3 \,.\, 10^5 t$, this merely tells us that $t = 1$ and $t' = 3 \,.\, 10^5$. It is then rather astonishing to be told that $3 \,.\, 10^5$ km. $= 1$ sec.; which—as we have explained the appendages km. and sec.—would be equivalent to $3 \,.\, 10^5 = 1$. But it is still more astonishing to find $\sqrt{-1}$ instead of 1 on the right-hand side.

The essence of this postulate can be clothed mathematically in a very pregnant manner in the mystic formula: $3 \,.\, 10^5$ km. $= \sqrt{-1}$ sec. —Minkowski, in *The Principle of Relativity*, 1923, p. 88.

The symmetry between space and time is so complete that one is justified in writing down the correct dimensional equation: 186,300 miles $= \sqrt{-1}$ seconds.—Birkhoff, *Origin, Nature and Influence of Relativity*, 1925, p. 59.

We shall not measure time in ordinary seconds but in terms of a mysterious unit equal to a second multiplied by $\sqrt{-1}$. . . . If we are asked why we adopt these weird methods of measurement, the

answer is that they appear to be nature's own system of measurement.—Jeans, *Mysterious Universe*, 1930, p. 110.

Now the use of $i \equiv \sqrt{-1}$ as an analytical expedient in physics is quite intelligible. For example, the equation

$$\partial^2\varphi/\partial x^2 + \ . \ . \ . - c^{-2}\partial^2\varphi/\partial t^2 = 0$$

becomes

$$\partial^2\varphi/\partial x_1^2 + \ . \ . \ . + \partial^2\varphi/\partial x_4^2 = 0,$$

if we put $x_1 = x$, $x_2 = y$, $x_3 = z$, $x_4 = ict$. We can then say that the equation is the four-dimensional analogue of the ordinary $\nabla^2\varphi = 0$. This is often convenient and must be understood in a purely analytical sense, it lightens our work by allowing us to use spatial metaphors. But it is devoid of physical significance, and the interim operator i cannot appear in our final measures.

However, these quotations profess to go far beyond this. The difficulty is to ascertain what precisely the authors think they are saying. It is bad enough to be told that so many miles are equal or identical with so many seconds; to say that they are equal to the root of minus one times so many seconds is to make a statement devoid of all intelligible content. The only hints at explaining the equation $3 \,.\, 10^5$ km. $= i$ sec.—which involves the equation 1 sec. $= - i \,.\, 3 \,.\, 10^5$ km.—are the following: it is a mystic formula, it is a correct dimensional equation, it reproduces nature's own system of measurement. So we can choose between mysticism, dimensions and Nature!

Rignano [49] is quite justified in saying that 'this mathematical mysticism recently has reappeared with renewed vigour in Einstein's theory of relativity,' which, while analytically ingenious, 'has at the same time demonstrated all its dangerousness in predisposing mathematicians to mystical conceptions very prejudicial to the clear understanding of the real state of affairs.' There is little doubt that the same type of mind which revels in 'dimensions' finds expression in 'welding' time and space and in attributing magic to the root of minus one.[50]

Let us now change from c.g.s. units to units of x cm., y gm., z sec. The velocity of light $c = 3 \,.\, 10^{10}$ becomes c' and the gravitational constant $\gamma = 6{\cdot}664 \,.\, 10^{-8}$ becomes γ'. From $f = \gamma mm'/r^2$,

[49] *The Psychology of Reasoning*, 1923, p. 185 f.

[50] 'The theory of relativity is only the latest phase of a critical principle which issued earlier in the method of dimensional analysis and in the applications of functional equations to the basic axioms of dynamics.'—Temple, p. 11.

we have $[\gamma] = L^3/MT^2$. Hence, from our general formula (14.6a) for the change of units,

$$c = c'x/z, \quad \gamma = \gamma' x^3/yz^2.$$

Assume $c' = \gamma' = 1$, take as units: kilometre ($x = 10^5$), cogram and cosec, the latter being merely names for y gram and z second. Clearly

$$1/T = z = x/c = 1/3 \times 10^{-15}$$
$$1/M = y = xc^2/\gamma = 1{\cdot}35 \times 10^{33}.$$

Now the mass of the sun is $m = 1{\cdot}985 \times 10^{33}$ gram. Hence $m' = Mm = m/y = 1{\cdot}47$ cogram. This result is expressed by relativists as follows: [51]

> Gravitational mass is a length, e.g. the mass of the sun is 1·47 kilometres.—B. Russell, *Analysis of Matter*, 1927, p. 341.
>
> In Einstein's theory of gravitation the mass (causing the gravitational field) appears as a length.—Weyl, *Philosophie der Mathematik und Naturwissenschaft*, 1927, p. 103.
>
> For the sun the quantity *m*, called the gravitational mass, is only 1·47 kilometres; for the earth it is 5 millimetres.—Eddington, *Space Time and Gravitation*, 1920, p. 98.
>
> It is usual to adopt a system of units in which the constant of gravitation and the velocity of light in space devoid of gravitation are each unity and the unit of length is the kilometre. The two fundamental unit quantities being regarded as dimensionless constants, the dimensions of mass and length become identical and mass is measured in kilometres.—W. B. Morton, PM 42 (1921) 512.

This language is so peculiar that even the late Sir George Greenhill was puzzled:

> In these c.g.s. units Einstein's *m* must denote a length, in centimetres. It is mysterious then that Einstein is quoted as calling *m* the mass of the sun, as if a mass could be measured in centimetres, by a metre rule, and not in grams. Some mysterious unexplained astronomical units must have been employed, and writers should enlighten us on this point of the theory.—PM 41 (1921) 145.

We shall try to supply the enlightenment which was not then forthcoming. In the first place, the matter has not the remotest connection with the theory of relativity; it is merely an elementary problem in changing units. In the second place, it is nonsense to speak of measuring mass in kilometres; the mass

[51] Even elementary students are now supposed to know in examinations that the mass of the sun is 1·47 kilometres.—W. Bond, *Numerical Examples in Physics*, 1931, p. 36.

is measured in cograms, each of which is $1 \cdot 35 \times 10^{33}$ grams. Numerically $m' = \gamma m/10^5 c^2$, where m is in grams. Or, putting it otherwise, we easily see that $\gamma m/c^2 r$ is tautometric, and therefore equal to m'/r', since $\gamma' = c' = 1$.

Hence arose the peculiar idea that m' is measured in the same units as r'. The mistake is due to the delusion that γ and c are 'regarded as dimensionless constants,' i.e. as having the measure-ratio unity, whereas in fact the measure-ratios are $\gamma/1$ and $c/1$. This pseudo-mystic attitude to dimensions also explains such mysterious statements as the following:

> If the gravitational constant of Newtonian mechanics is regarded as a mere number, mass has the dimensions $L^3T^{-2} = L$ if L and T have the same dimensions [i.e. if $L = T$].—F. Murnaghan, PM 43 (1922) 585 note.
>
> There is a relation between the three fundamental concepts of classical physics, which can be written thus: Mass = Space : Time squared. Hence already in classical mechanics these three ideas are not independent.—R. Lämmel, *Die Grundlagen der Relativitätstheorie*, 1921, p. 128.
>
> The theory of relativity has lessened the number of the fundamental units by the discovery of inner connections between them. For example, the time-unit loses its self-sufficiency owing to the unification of space and time and reduces to the unit of length, on the ground of the natural unit: velocity of light = 1, thus 1 sec. = $3 \,.\, 10^{10}$ cm. Furthermore, owing to the connection discovered by the general theory of relativity to exist between mass-density and space-curvature, mass is reduced to the unit of length, on the ground of the relation: 1 gram = $1 \cdot 86 \times 10^{-27}$ cm. Thus two universal constants are eliminated by understanding their true meaning.—C. Lanczos, *Ergebnisse der exakten Naturw.* 10 (1931) 113.

We have already dealt with the absurd equation which makes a second of Duration to be so many times a centimetre of Length, we have also protested against making a centimetre to be so many grams. There is no difficulty whatever in 'eliminating' c and γ, i.e. in choosing units of measurement so that $c' = \gamma' = 1$. This is effected by an elementary process accessible to any schoolboy, without any reference to the alleged unification of space and time or to the connection of mass-density and space-curvature. There is, says Planck,[52] 'nothing to prevent our choosing the unit of mass so that $\gamma = 1$. The gravitational constant would then be a pure number, and the mass would not be a self-dependent quantity but would have the dimensions $[l^3/t^2]$.' We cannot

[52] *General Mechanics*, 1932, p. 46.

admit these conclusions; we have maintained that γ is always a pure number. The fact that, with certain basic units, it turns out to be unity, is trivial and irrelevant. Ths equation $M = L^3/T^2$ merely means that $\gamma/\gamma' = 1$, i.e. γ has the same value in the two systems of units. All this talk of mass = volume/time^2 and the like is without meaning.[53]

We have already protested against the rather cool assumption of relativists that somehow they have acquired proprietary rights over the set of units in which $c' = \gamma' = 1$. The subject has no connection with Einstein's theory. The fact that a writer upholds that theory gives him no right whatever to indulge in loose and inaccurate terminology concerning elementary matters like changes of units. Sir Arthur Eddington however appears to think otherwise:

> Objection is sometimes taken to the use of a centimetre as a unit of gravitational (i.e. gravitation-exerting) mass. But the same objection would apply to the use of a gram, since the gram is properly a measure of a different property of the particle, viz. its *inertia*. Our constant of integration m is clearly a length; and the reader may, if he wishes to make this clear, call it the gravitational radius instead of the gravitational mass. But when it is realised that the gravitational radius in centimetres, the inertia in grams, and the energy in ergs, are merely measurements in different codes of the *same* intrinsic quality of the particle, it seems unduly pedantic to insist on the older discrimination of these units which grew up on the assumption that they measured qualities which were radically different.—*Mathematical Theory of Relativity*, 1924[2]. p. 87.

It is very difficult to argue against a person who claims to possess such an intimate knowledge of nature. Perhaps it is true that when we compare one Length with another and call the ratio so many centimetres, when we say the mass of a body is so many times that of another which we choose to call unity (1 gram), and when we measure energy or work in ergs, we are merely using different 'codes' for comparing 'the same intrinsic quality of the particle' to some other specific instance of this mysterious intrinsic quality. All this may have some meaning *in rerum natura*—but not in a laboratory, not in experimental

[53] The objection of Jeans (p. 14) to $M = L^3/T^2$ is based on this misinterpretation of dimensions: 'As a matter of fact, however, we know that mass is something entirely apart from length and time, except in so far as it is connected with them through the law of gravitation.'

science. It is not just 'pedantic' to discriminate between the measures of length, mass and energy; it is a vital necessity, distinguishing physics from algebra. The real pedantry occurs when a relativist, just because he happens to use m instead of $l = \gamma m/c^2$ as a 'constant of integration,' is willing to reduce physics to chaos rather than change a letter of the alphabet.

Our discussion of γ and its measure-ratio is by no means irrelevant to electromagnetics. Witness this quotation from an authoritative American symposium of 1933:

> Being primarily a mathematician, Gauss simplified his formulas as much as possible by making the inverse-square law for magnetic poles factor-free. This was good mathematics but poor physics. It equated the product of two magnetic poles to the product of a force and the square of a distance. There was no justification for this at that time, nor is there any at the present time; it has been the source of much confusion. . . . The reduction of magnetism to mechanics by a factor-free inverse-square law is not justified.—G. A. Campbell, p. 70.

It would be hard to beat this as a concentrated and complete misunderstanding of the symbols of elementary physics. Apparently there is no objection to the law $f = mm'/\beta r^2$; but for some unexplained reason, Gauss made a shocking mistake in taking $\beta = 1$. Apparently astronomers are also mistaken in using units for which $\gamma' = 1$. At this stage of our discussion it is no longer necessary to argue against this absurd position. So long as such views are seriously maintained concerning physical symbols and dimensions, it is futile to expect clarity or utility in discussions on the units or the 'nature' of electromagnetic quantities.

8. 'Natural' Units.

Let us start with the values of the following constants in c.g.s. units:

Quantity	*C.g.s. value*	*Measure-ratio*
Constant of gravitation	$\gamma = 6{\cdot}664 \times 10^{-8}$	L^3/MT^2
Velocity of light	$c = 2{\cdot}99796 \times 10^{10}$	L/T
Planck's constant	$h = 6{\cdot}547 \times 10^{-27}$	ML^2/T

Then if we adopt units of x gram, y cm., z sec., so that the new values are unity ($c' = \gamma' = h' = 1$), we have by (14.6a),

$$\gamma = y^3/xz^2, \quad c = y/z, \quad h = xy^2/z.$$

Or

$$x = c^{\frac{1}{2}}h^{\frac{1}{2}}\gamma^{-\frac{1}{2}}, \quad y = c^{-\frac{3}{2}}h^{\frac{1}{2}}\gamma^{\frac{1}{2}}, \quad z = c^{-\frac{5}{2}}h^{\frac{1}{2}}\gamma^{\frac{1}{2}}.$$

This gives approximately

$$x = 5 \cdot 4 \times 10^{-5}, \quad y = 4 \cdot 0 \times 10^{-33}, \quad z = 1 \cdot 3 \times 10^{-43}.$$

Let us now add

Boltzmann's gas-constant : $k = 1 \cdot 372 \times 10^{-16}$: $ML^2/T^2\,\Theta$.

We can choose a new unit of temperature u Centigrade degrees so that the new $k' = 1$. We have

$$k = xy^2/z^2u, \text{ or } u = kz^2/xy^2 = 2 \cdot 4 \times 10^{32}.$$

We have thus arrived at Planck's famous 'natural units.'

> We have the means of establishing units of length, mass, time and temperature, which are independent of special bodies or substances, which necessarily retain their significance for all times and for all environments, terrestrial and human or otherwise, and which may therefore be described as 'natural units.'—Planck, *Theory of Heat Radiation*, Eng. tr. Philadelphia, 1914, p. 84.
>
> This is the most perfect hitherto conceivable system of measurement, and also the one which most deserves the name of absolute.—F. Auerbach, *Die Methoden der theoretischen Physik*, 1925, p. 12.

Now the choice of units is entirely a question of practical convenience ; it must be decided by the men in the laboratory and in the factory. It has no theoretical significance. We have already pointed out that the ratio of two conspecific quantities is independent of the units employed, and that every general physical equation is a relation between tautometric products. There is therefore justification for Dr. N. Campbell's outspoken criticism (vii. 396) : 'I can find no word milder than ridiculous to characterise Planck's suggestion.'

We must also reject Eddington's assertion [54] : 'It is evident that this length [y cm.] must be the key to some essential structure.' Surely it is anything but evident. Pointing to the new unit of temperature-difference ($2 \cdot 4 \times 10^{32}$), Prof. Bridgman says (iii. 102) :

> In the wildest speculations of the astrophysicists, no such temperature has ever been suggested ; yet would Prof. Eddington maintain that this temperature must be the key to some fundamental cosmic phenomenon ?

Moreover, other systems of 'natural' units have been proposed.

[54] *Report on the Relativity Theory of Gravitation*, 1920², p. 91.

> A truly natural system of physical units would be one which was based on the electron or a [an integral ?] multiple of it as unit of electric quantity, the velocity of light or a fraction [an aliquot part ?] of it as unit of velocity, and the mass of an atom of hydrogen or a [an integral ?] multiple of it as unit of mass.—J. A. Fleming, *Enc. Brit.* 27 (1911[11]) 745; repeated by A. W. Porter in 14th edition (xvii. 879 f.).

Still another system, called rather pompously 'ultimate rational units,' has been proposed by Lewis and Adams. Let us call these l cm., m gram, t sec., θ degrees C. These are determined as follows:

(1) The cm. is retained, i.e. $l = 1$.

(2) The velocity of light is to be unity, $c' = 1$, i.e. $l/t = c$ or $t = 1/c$.

(3) 'The electrostatic electron charge' is to be $1/4\pi$. Expressed more accurately, this means that we start with the elst system ($\alpha = 1$) and the electronic charge $q = 4\cdot 770 \times 10^{-10}$ elst, and that we take $q'\alpha'^{\frac{1}{2}} = 1/4\pi$. Apparently it is intended to take $\alpha' = 1$, but nothing is said about β'. Since

$$Q = M^{\frac{1}{2}}L^{\frac{3}{2}}[\alpha^{\frac{1}{2}}]T^{-1},$$

we have from (14.6a)

$$q = q'\alpha'^{\frac{1}{2}}m^{\frac{1}{2}}l^{\frac{3}{2}}/t,$$

or

$$m = (4\pi q/c)^2 = 1\cdot 39 \times 10^{-37}$$

since $l = 1$, $t = 1/c$, $q'\alpha'^{\frac{1}{2}} = 1/4\pi$. The only reason given by Lewis (p. 741) for this peculiar choice of $q'\alpha'^{\frac{1}{2}}$ is this: 'This decision, which seems arbitrary, was reached after looking ahead to some of its consequences.'[55]

(4) The Boltzmann constant is to be unity. Hence

$$k = ml^2/t^2\theta, \text{ or } \theta = (4\pi q)^2/k = 1\cdot 66 \times 10^{-2}.$$

The first argument adduced for this system is as follows:

> In the system that I outlined, not only do the various units and dimensions of electric and magnetic quantities become identical, but the dimensions of all electric and magnetic quantities become integral—thus doing away with the fractional dimensions which are now ascribed to the majority of these quantities and which many have felt to be a blemish upon the existing system of dimensions.—Lewis, ii. 748.

[55] Lewis says, PM 45 (1923) 270: '1 gram $= 2\cdot 499 \times 10^{37}$ reciprocal centimetres'!

We need not waste time with this statement. It happens to be untrue; and if it were true, it would be worthless. A procedure (change of units) which presupposes and is based upon dimensions (measure-ratios) cannot result in altering these dimensions!

Before examining further claims made for these units, it will be helpful to enumerate some tautometric products concerning which there has been considerable discussion.

The first is

$$A = ch/2\pi q^2 = 137 \text{ approximately.}$$

Here h is Planck's quantum, q is the electronic charge in elst, and 2π is inserted for certain reasons of convenience. It is easily seen that the measure-ratio of this quantity is not 1 but $[\alpha^{-1}]$. Hence the more accurate expression is

$$A = \alpha ch/2\pi q^2,$$

where of course $\alpha = 1$ for elst measure. There is a slight uncertainty in the numerical value owing to probable errors in h and q. Bond [56] gives

$$137{\cdot}01_7 \pm 0{\cdot}05_9.$$

According to Jeans,[57] 'it is significant that ch is of the same physical dimensions as q^2, and so may be regarded as being the same thing as q^2 except for a numerical multiplier.' We know nothing about this 'significance'; we cannot imagine what kind of a 'thing' the square of a charge is, if it is not a number; and the only truth contained in the statement that ch is 'the same thing' as q^2 is that they are *not* the same number. We had better keep to the simple statement that $\alpha ch/q^2$ is tautometric.

The second tautometric product is

$$B = \sigma h^3c^2/k^4$$
$$= \text{approximately } 41.$$

Here σ is Stefan's constant, which occurs in the law: total emissive power of a black body $= \sigma \times$ (abs. temp.)^4. In c.g.s. units

$$\sigma = 5{\cdot}709 \times 10^{-15} \text{ erg/cm.}^2 \text{ sec. grad}^4.$$

The energy density is $a\theta^4$, where θ is now used to denote the absolute temperature, and

$$a = 4\sigma/c = 7{\cdot}617 \times 10^{-15} \text{ erg/cm.}^3 \text{ grad}^4.$$

[56] PM 12 (1931) 635. According to Millikan, PR 35 (1930) 1231, the best value is 137·29.

[57] *Nature* 115 (1925) 365*.

The third tautometric product is

$$C = ck^4/\sigma q^6 = \text{approx. } 16 \times 10^6.$$

But this is not independent, for

$$2\pi A = (BC)^{\frac{1}{2}}.$$

Returning now to Lewis-Adams units, we have

$$\sigma = \sigma' m/t^3\theta^4.$$

Whence we find at once

$$\sigma' = (4\pi)^6/C.$$

This is in furtherance of their expectation that 'all universal constants will prove to be pure numbers, involving only integral numbers and π' (p. 97). But there is nothing at all in this argument, for the formula for σ' is an immediate deduction from

$$\sigma = ck^4/q^6C.$$

They now assume that $\sigma' = 1/4$. The assumption may be made simpler by putting $\sigma = ac/4$ and $\sigma' = a'c'/4$, so that the assumption is equivalent to taking $a' = 1$. We have therefore

$$C = 4(4\pi)^6 = 15{\cdot}61 \times 10^6,$$

which might have been put forward as a direct suggestion, without any changing of units.[58] Or

$$\sigma = ck^4/4(4\pi q)^6 = 5{\cdot}70 \times 10^{-5}.$$

Similarly

$$h = h'ml^2/t = h'(4\pi q)^2/c$$

or

$$h' = A/8\pi,$$

which also follows at once from $h = 2\pi Aq^2/c$. It is now assumed that

$$\begin{aligned} A &= 8\pi(8\pi^5/15)^{\frac{1}{3}} \\ &= 137{\cdot}348. \end{aligned}$$

This is a rather complicated assumption, and moreover it has nothing to do with these special units. Let us make it somewhat clearer. According to Planck's theory [59]

$$B = 2\pi^5/15.$$

[58] Lewis says, PM 45 (1923) 271: 'We were able to calculate a value of Stefan's constant claiming a much higher accuracy than the values which had been obtained by experiment' (average $5{\cdot}81 \times 10^{-5}$). But Lewis and Adams (p. 99) admitted that Coblentz had previously given $5{\cdot}7 \times 10^{-5}$ as the probable value.

[59] *Theory of Heat Radiation*, 1914, p. 171 (where $a = \pi^4/90$).

It follows that

$$\begin{aligned} A &= (BC)^{\frac{1}{2}}/2\pi \\ &= 8\pi(8\pi^5/15)^{\frac{1}{2}}, \end{aligned}$$

if we assume $C = 4(4\pi)^6$. Hence the net contribution of Lewis and Adams is this assumption about C. Their 'ultimate rational units' are unnecessary and irrelevant.[60]

If we assume that h is in theory dependent on q and c, and (as Planck did) that σ depends on h, k and c, then the products A and B can involve only operational numbers, i.e. numbers introduced by some such process as statistical summation and integration. Various suggestions, mostly empirical guesses, have been made about A, several involving the masses of the proton and electron, but none of them is satisfactory.[61]

We may now briefly refer to 'the chemical constant' in connection with its 'dimensions' and with Lewis-Adams units. But of course for details of theory and practice we must refer to the appropriate text-books; we are concerned here only with a minor point. For a gas at a very low temperature, when all gases behave as monatomic, we have [62]

$$\ln p = -jw\lambda_0/r\theta + \ln\theta^{\frac{5}{2}} + \ln a,$$

where p is the pressure, j the mechanical value of heat, w the atomic weight, $r = k/N$ where N is Avogadro's number $6{\cdot}064 \times 10^{23}$, θ is the absolute temperature, and $\ln a$ is called the chemical constant. Suppose a depends on $m = w/N$ the mass of the atom, Boltzmann's k and Planck's h. Then, since

[60] Their value of h is $6{\cdot}560 \times 10^{-27}$. 'Unless therefore we are to assume a bizarre coincidence, the recent determinations of the constants of Stefan and Planck furnish a striking justification of the ideas which Dr. Adams and I advanced.'—Lewis, PM 45 (1923) 272. But their only idea was to combine $C = 4(4\pi)^6$ with Planck's B.

[61] Fürth, PZ 30 (1929) 895 : $2\pi A = 15/32 \,.\, (M + m)^2/Mm$, where M and m are the masses of the proton and electron. J. Perles, *Naturw.* 16 (1928) 1094 : $2\pi A = M/m(\pi - 1)$. E. Witmer, PR 42 (1932) 316 : $A = 16/5 \,.\, [(7/2)^3 + (2/7)^3] = 137{\cdot}275$. Eddington has propounded a very transcendental theory. First he found $A = 136$ and declared : 'Although the discrepancy is about three times the probable error attributed to the experimental value, I cannot persuade myself that the fault lies with the theory.'—PRS 122A (1929) 358. Next he found 137.—PRS 126A (1930) 696. Observe also that γm^2 is of the same order as $q^2/4c^4$, showing a possible connection between electromagnetics and gravitation (see Ritz, p. 518).

[62] J. R. Partington, *Chemical Thermodynamics*, 1924, pp. 219, 259.

obviously $a\theta^{\frac{5}{2}}$ has the measure-ratio of pressure (M/LT^2), we easily find that

$$a = \text{constant } m^{\frac{3}{2}}k^{\frac{5}{2}}h^{-3}.$$

And in fact the quantum theory gives the constant to be $(2\pi)^{\frac{3}{2}}$. It is however also given [63] as $(4\pi)^{\frac{3}{2}}/e$, where e is the base of the natural logarithms. The difference is slight: $\ln(2\pi)^{\frac{3}{2}} = 1\cdot197$ and $\ln[(4\pi)^{\frac{3}{2}}e^{-1}] = 1\cdot215$, i.e. e, the base of the natural logarithms, is approximately $2^{\frac{3}{2}}$.

According to the quantum theory the entropy of an ideal monatomic gas is

$$S = r\ln[bv_m\theta^{\frac{3}{2}}m^{\frac{3}{2}}],$$

where $v_m = v/N$, $m = w/N$, and

$$b = (2\pi k)^{\frac{3}{2}}e^{\frac{5}{2}}h^{-3}.$$

Hence

$$S = r\ln[Cv\theta^{\frac{3}{2}}w^{\frac{3}{2}}],$$

where $C = bN^{-\frac{5}{2}} = 3\cdot836 \times 10^{-3}$. Lewis [64] tells us that 'according to the theory of ultimate rational units,' $b' = 1$ 'in the new units.' He infers that C is

$$k^{\frac{3}{2}}c^3/N^{\frac{5}{2}}(4\pi q)^6 = 3\cdot252 \times 10^{-3},$$

and that this has been since confirmed by experiment. However this is not equal to C, its correct numerical value is $8\cdot193 \times 10^{-3}$. We shall now show that b' is not unity. The measure-ratio of b is

$$[b] = [k^{\frac{3}{2}}h^{-3}] = 1/M^{\frac{3}{2}}\Theta^{\frac{3}{2}}L^6,$$

and $L = 1$. Hence, using m gram and θ grad for Lewis-Adams units,

$$\begin{aligned} b &= b'/m^{\frac{3}{2}}\theta^{\frac{3}{2}} \\ &= b'c^3k^{\frac{3}{2}}/(4\pi q)^6. \end{aligned}$$

Hence

$$b' = (2^{21}\pi^9e^5)^{\frac{1}{2}}A^{-3}.$$

If $b' = 1$, we should have

$$A = 2^{\frac{7}{2}}\pi^{\frac{3}{2}}e^{\frac{5}{6}} = 144\cdot9,$$

which is incorrect. Thus the 'theory' of ultimate rational units fails also in its final test. And indeed it must now be obvious,

[63] Nernst, *The New Heat Theorem*, 1926, p. 205.

[64] PR 18 (1921) 121; PM 45 (1923) 273. 'It has recently been shown by Lewis that the constant C can be calculated, with an accuracy far greater than can yet be attained by experiment, from other well-known constants of nature.' —G. N. Lewis, G. Gibson and W. Latimer, *J. Am. Chem. Soc.* 44 (1922) 1009.

in view of our discussion, that no theoretical result whatever can possibly emerge from a mere change of units, though certain suggestions can be based on a consideration of tautometric products.

To the above remarks (already in print) I feel tempted to add a brief reference to the use of ' dimensionless constants ' in recent cosmogonical speculation. At the start it is taken for granted that certain astronomical observations imply that the spiral nebulae are receding with velocities proportional to their distances from us. This alleged fact sounds a bit far-fetched and should be received with caution. As Miss Janet Clark pointed out, if the speed of recession increases with distance it must also be taken as increasing as one goes backwards in time. Whereupon Sir Arthur Eddington declared his standpoint as follows :

> My own point of view has been that the distribution of nebular motions *taken by itself* is a phenomenon which would admit of an almost unlimited number of cosmogonic interpretations ; and I have no objection to admitting yet another. Some of the interpretations offered seem to me to lack plausibility ; but I have long since found that there is no accounting for tastes in such a matter. The position is quite different for those of us who approach the phenomenon by way of pure physical theory. Independently of any astronomical observations, we are more or less convinced of the existence of cosmical repulsion as a necessary consequence of the relativity of our measurements.—*Nature* 132 (1933) 406.

Such frankness is extremely refreshing. It appears that the adherents of pure physical theory—so pure that it is indistinguishable from *a priori* mathematical speculation—are already convinced quite independently of any observations ! Anyway, let us record the judgement of Dr. Silberstein (*Causality*, 1933, p. 137) that ' the relativistic theory of the expanding universe . . . is far from being firmly established. It certainly is beset with serious difficulties.'

Prescinding from the question of fact, let us agree with Dirac :

> The recession of the spiral nebulae with velocities proportional to their distances from us requires, if we assume these velocities to be roughly constant [a peculiar assumption !], that at a certain time in the distant past all the matter in the universe was confined within a very small volume. This time appears as a natural origin of time and provides us with a zero from which to measure the epoch of any event. Referred to this zero, the present epoch, according to Hubble's data, is about 2×10^9 years.—P.A.M. Dirac, PRS 165A (1938) 200.

That is, the time elapsed since everything was packed together is $t = 6.3 \times 10^{16}$ seconds. Which statement, of course, implies the objectivity of Duration independently of measurement. 'Let us express this,' says Dirac, 'in terms of a unit of time fixed by the constants of atomic theory.' For which he takes

$$t_0 = q^2/mc^3 = 10^{-23} \text{ sec.},$$

where q is the electronic charge in elsts, m the electronic mass in grams, and c the velocity of light in cm. per sec. Of course, we might have taken a different 'natural unit' such as q^2/Mc^3 where M is the mass of the proton; or we could use At_0 i.e. $h/2\pi mc^2$, which is $137t_0$. And Ct_0 would be $16 \times 10^6 t_0$. Anyway t/t_0 is about 6×10^{39} (Dirac says 7×10^{38}). So we look around and see if we can get another tautometric number of this magnitude. Let D be the ratio of the electric to the gravitational force between the electron and the proton:

$$\begin{aligned} D \equiv f/f' &= (q^2/r^2)/(\gamma Mm/r^2) \\ &= q^2/\gamma Mm \\ &= 2.3 \times 10^{39}. \end{aligned}$$

We conclude that

$$t/t_0 \to f/f' \to 10^{39}.$$

This is expressed by Dirac as follows (p. 201):

> We see there is a close agreement between the present epoch, expressed in atomic units, and the ratio of the gravitational to the electric force between two elementary particles. Such a coincidence we may presume is due to some deep connexion in Nature between cosmology and atomic theory. Thus we may expect it to hold not only at the present epoch but for all time, so that for example in the distant future when the epoch is 10^{50} we may expect D will then be of the order 10^{50}.

We need not follow Dirac in his further generalisation, nor need we inquire what was D when the 'epoch' was zero or very small. It is sufficient to append some brief criticisms of the foundation of this speculation which is reminiscent of the musings of Kepler.

(1) It is clear that t is taken not as a date but as the measure of a relevant physical duration. Dirac presently abandons the hypothesis that the velocity of recession of each spiral nebula is roughly constant. But, he says (p. 201), 'without this assumption we can still talk about the epoch of an event, but we have no natural zero from which to measure it, so that only the difference of two epochs can enter into the laws of nature.' Later on

(p. 203) he concludes that the velocity varies as $t^{-2/3}$. ' With this law of recession we still have a natural origin of time, namely the zero of the t, when all the nebulae were extremely close together '—and, let us add, moving with infinite velocity [relative to what ?]

Now why should closetogetherness have such an extraordinary significance ? Whether we call the quantity t or $t - t'$, it must, since it occurs in a physical law, represent the duration of a physical process, some continuously operative cause. And we are supposed to arrive at this mysterious law not by observation but by number-juggling.

(2) As for ' the atomic unit ' t_0, there is no such thing at all. We simply have the logometric formula

$$[q^2/mc^2] = T[\alpha]$$

That is, the measure-ratio of the quantity is T, the ratio of the time-measures or the inverse ratio of the time-units employed, multiplied by the ratio of the α-coefficients used (which latter Dirac conveniently takes to be unity). Similarly the measure-ratio of surface-tension (s) is $S = M/T^2$, so that

$$[m^{\frac{1}{2}}/s^{\frac{1}{2}}] = T,$$

where m is the mass of the body or of a molecule. Are we to infer that $\sqrt{m/s}$ is a natural molecular unit of time ? The invalidity of such reasoning will be made clear when in the next chapter (p. 790 f.) we discuss electrical resistance as ' velocity. From the fact that a measure-ratio is T it is illegitimate to infer that a ' natural unit ' of duration is involved.

(3) The importance attached to the approximate equality of t/t_0 and f/f' is more akin to superstition than to science. The quantity t, a duration extending aeons into the unknown past, is reached by a very precarious argument ; the quantity t_0 should really be designated q^2/mc^2 and has no durational significance whatever. And f/f' is the ratio of two simultaneous forces acting here and now. The equation is expressed by saying that

$$\gamma c^3 M m^2 t/q^4$$

is approximately unity. And in this guise the statement appears to be rather innocuous. To infer that the collocation of quantities always remains unity from $t = o$ (or $-\infty$?) to $t = \infty$ is just a specimen of arithmolatry.

(4) How happy are those who can thus so easily find the deep connexion in Nature (spelt with a capital letter) between cosmogony and atoms! Fortunately there are to-day other speculators abroad who, also by the wizardry of numbers, discover a different constitution in Nature.

> Milne has suggested that, if a suitable model for the universe can be constructed out of such general principles as have here been considered, it is reasonable to assume that it should not be necessary to introduce any constants having physical dimensions.—A. G. Walker, *Proc. Lond. Math. Soc.* 42 (1936) 121.

That is, in the new-fangled cosmogonical speculations, the only tautometric number which can occur is what Walker calls 'a pure constant'—whatever that means. The only hope for experimental physics is that the apriorists will destroy one another.

9. Operations and Concepts.

As far back as 1870 Maxwell wrote (iv. 217):

> As science has been developed, the domain of quantity has everywhere encroached on that of quality, till the process of scientific inquiry seems to have become simply the measurement and registration of quantities, combined with a mathematical discussion of the numbers thus obtained.

There is an obvious truth in this, provided we do not, as is commonly done, distort it into the assertion that science reduces qualities to quantities. For physics does not transform quality, it merely prescinds or abstracts from it. The purely numerical equation $v = l/t$ does not in any sense reduce Space, Time and Motion to generic quantity.

Physics deals with certain quantitative interrelationships occurring in a complex qualitative process; so far from denying this background or context, the equation is devoid of physical significance unless it be presupposed.

It seems to be held generally to-day that the recognition of 'physical quantities' as measure-numbers is somehow due to the development of Einstein's theory. So far is this from being the case, relativists are among the greatest offenders in employing and misapplying 'dimensions.' But a great parade is made of ridiculing alleged 'absolute' definitions, such as that of the number

m as 'the quantity of matter.' So Eddington[65] assures us solemnly that 'distance is defined by certain operations of measurement and not with reference to nonsensical conceptions such as the 'amount of emptiness' between two points.' While the man of straw is thus demolished, Eddington's own position is not quite clear. Elsewhere he tells us :

> The vocabulary of the physicist comprises a number of words—such as length, angle, velocity, force, work, potential, current, etc.—which we shall briefly call physical quantities. . . . Physical quantities are defined primarily according to the way in which we recognise them when confronted by them in our observation of the world around us. . . . To find out any physical quantity we perform certain practical operations followed by calculations. . . . The physical quantity so discovered is primarily the result of the operations and calculations ; it is, so to speak, *a manufactured article*, manufactured by our operations. But the physicist is not generally content to believe that the quantity he arrives at is something whose nature is inseparable from the kind of operations which led to it. . . . By finding that he can lay x unit measuring-rods in a line between two points, he has manufactured the quantity x which he calls the distance between the points. But he believes that that distance x is something already existing in the picture of the world—a gulf which would be apprehended by a superior intelligence as existing in itself without reference to the notion of operations with measuring-rods.—*Mathematical Theory of Relativity*, 1924[2], p. 1.

So, according to Eddington, the non-relativist is supposed to believe that the number $x = A/B$, where A is the measured Length and B is the unit Length chosen, is 'something already existing' independently of B. No one holds such an absurd view. 'The physicist,' he argues, 'would say that he *finds* a length and *manufactures* a cubic parallax ; but it is only because he has inherited a preconceived theory of the world that he makes the distinction.' Here again a man of straw is set up to talk nonsense. Without leaving length-measure, we can say that the numbers x, x^2, x^3 are equally justifiable as numbers ; their scientific appropriateness varies ; in the case of gravitational attraction x^2, not x, is found to occur. It is extremely difficult to decide whether relativists are uttering the truism that $x = A/B$ is arbitrary and depends on B, or whether they are paradoxically asserting that A and B have no objective existence at all. For instance, what

[65] *Nature of the Physical World*, 1928, p. 222. Millikan says (iii. 185) : 'Inertia is the only invariable property of matter. It is the quantitative measure of matter, and matter quantitatively considered is called *mass*.'

does Eddington mean when he says (p. 141) : ‘ There is no such thing as absolute length, we can only express the length of one thing in terms of the length of something else ’ ? Or let us quote Bridgman (i. 6), substituting A for Time in order to generalise the argument :

> We do not understand the meaning of absolute A unless we can tell how to determine the absolute A of any concrete event, i.e. unless we can measure absolute A. Now we merely have to examine any of the possible operations by which we measure A to see that all such operations are relative operations. Therefore the previous statement that absolute A does not exist is replaced by the statement that absolute A is meaningless.

Let us avoid the adjectives absolute and relative, which apparently evoke prejudice. The argument then is that A is ‘ meaningless ’ because measurement gives only A/B ; and now its weakness is apparent. If A is x_1 relative to B_1 and x_2 relative to B_2, neither x_1 nor x_2 is an absolute predicate of A. But if A had no independent character, it would not be x_1 relative to B_1 nor x_2 relative to B_2. Given a rod, its size in yards is determined by the yardstick ; but given the yardstick, the number of yards is determined by the stick itself. If the rod had not a determinate sizableness independent of the yardstick, or if the yardstick had no Length independently of the rod, the relation would be completely indeterminate.[66] The determinateness or measurability of a Length is independent of the procedure of measuring it. If it *meant* the operation, I could not think of a Length without thinking of the extension and displacement of the measuring-rod, and in this latter idea Length is already assumed. In general, the metric quality is logically prior to measurement, the relating implies relata. We cannot therefore admit Prof. Bridgman's contention (i. 69) that ‘ the concept of time is determined by the operations by which it is measured.’ He has refuted himself when he admitted elsewhere [67] that ‘ the time concept has to be assumed as primitive and unanalysable, for the operations essentially assume that the operator understands the meaning of later and earlier in time.’

Indeed Bridgman refutes himself in the very enunciation of his thesis (i. 68 f.) :

[66] *Cf.* C. I. Lewis, *The Mind and the World Order*, 1929, pp. 167–174.
[67] *Science*, 75 (1932) 424.

> According to our viewpoint, the concept of time is determined by the operations by which it is measured. . . . A metre stick is set up with mirrors at the two ends, and a light-beam *travels* back and forth between the two mirrors without absorption. The *time required* for a single passage back and forth is defined as the unit of time ; and time is measured simply by counting these *intervals*.

Is it not perfectly obvious, from the words we have italicised, that he assumes beforehand as an empirical datum the concept of time or duration which he professes to define by this rather crude and schematic operation ? His measuring-operation is merely a mechanism for obtaining the *ratiofication* of two Durations. Yet, it may be objected, 'there are many varieties of measurement, each of which gives rise to a different time-concept,'[68] e.g. earth-rotation, pendulum, tuning-fork. In that case, it is curious that the unsophisticated man identifies all these concepts, gives them the same meaning and calls them by the same name. Is it really necessary, when studying physics, to perpetrate such paradoxes as saying that the earth rotates in one time, a pendulum oscillates in another, I myself move in a third, and so on ? Besides its absurdity, no one really uses this plurification even in physics. It merely happens to be fashionable at the moment to write this kind of stuff.

We reject then the thesis of Bridgman (i. 8), according to which 'in general we mean by any concept nothing more than a set of operations, the concept is synonymous with the corresponding set of operations.' The operation itself is not haphazard, it is meaningful and purposive ; its sole object is to secure a ratio. Regarded without reference to this, simply as a material procedure, every operation is essentially accurate ; it is what it is. How then can Bridgman reconcile this with his admission (i. 33) that 'all results of measurement are only approximate' ? They certainly give *some* ratio ; and if this is *defined* by the operation, there can be no correction. The truth is that measurement is an operation describable only in terms of prior concepts ; and in the light of these it is legitimate to correct the results of its physical execution. The concept, say, of Length is not thereby altered or determined, only our estimate of the correct length-number is changed. Prof. Bridgman no more defines Length operatively than he defines Time :

[68] *Outline of Atomic Physics.* By Members of the Physics Staff of the University of Pittsburg. New York, 1933, p. 326.

> We start with a measuring-rod and lay it on the object, . . . then move the rod along in a straight-line extension of its previous position, . . . and call the length the total number of times the rod was applied (i. 9).

The whole phraseology is spatial. And this crude operation does not even constitute the meaning of *length*; it is merely a rough-and-ready way for estimating the value of what we know already to exist: the ratio of two Lengths.

In spite of its laboratory-vocabulary, this so-called operational viewpoint is not really a genuine product of physics; it is the echo, within the precincts of science, of that curious and typical American doctrine known as 'behaviourism.' In a similar way we find, as an importation into physics, that doctrine of 'idealism' which is a reaction against materialist philosophy. We shall take Sir Arthur Eddington as its exponent.

> We feel it necessary to concede some background to the measures—an external world; but the attributes of this world, except in so far as they are reflected in the measures, are outside scientific scrutiny.—*Nature of the Physical World*, 1928, p. xiii.
>
> What we are dragging to light as the basis of all phenomena is a scheme of symbols connected by mathematical equations. That is what physical reality boils down to when probed by the methods which a physicist can apply.—*New Pathways in Science*, 1935, p. 313.
>
> What do the symbols stand for? The mysterious reply is given that physics is indifferent to that; it has no means of probing beneath the symbolism.—*Science and the Unseen World*, 1930, p. 20.
>
> Everything known about the material world must in one way or another have been inferred from these stimuli transmitted along the nerves. . . . The inferred knowledge is a skeleton frame, the entities which build the frame being of undisclosed nature. For that reason they are described by symbols, as the symbol x in algebra stands for an unknown quantity. . . . Its substance has melted into shadow. None the less it remains a real world if there is a background to the symbols—an unknown quantity which the mathematical symbol x stands for. We think we are not wholly cut off from this background. It is to this background that our own personality and consciousness belong, and those spiritual aspects of our nature not to be described by any symbolism or at least not by symbolism of the numerical kind to which mathematical physics has hitherto restricted itself.—*Ibid.*, pp. 22 f., 24 f.

Here speaks the typical theorist. He deals with numbers which he prefers to call 'symbols.' This scheme of symbols is the basis of all phenomena, they constitute physical reality, they are 'the scientific world . . . a shadow-world, shadowing a world

familiar to our consciousness.' [69] Thus 'matter and all else that is in the physical world would have been reduced to a shadowy symbolism.' [70] But though matter is gone, apparently 'nerves' remain, for they transmit the stimuli from which we infer 'matter and all else'—or is it the 'symbols'? The typical 'mathematical symbol x stands for' 'an unknown quantity,' i.e. a number which has to be determined. Nevertheless, the 'background' is not numerical, it is spiritual.

We are not interested in the coherence or tenability of this view as a general philosophy. We are concerned only with the attempt to foist it upon us as the authoritative interpretation of physical science. It is becoming far too common for prominent physicists, with a gift for popularising, to gain adherence to their own brand of philosophy by representing it as the latest pronouncement of physics. We shall therefore, by way of counterblast, summarise the view maintained in this chapter, without in any way trespassing on the field of philosophy.

(1) Let us begin with practical physics. The laboratory—in which term we include the workshop, the factory, etc.—is the source and final test of the theorist's work. If the lab-man's task could be summed up in one word, it would be 'ratiofication,' typified by $x = A/B$ and complicated combinations of such numbers. The theorist's initial x thus originates, and his final test is thus verified or rejected. It may please the theorist, in moments of exaltation, to forget the humble origin and ultimate judgement of his symbolic career; especially nowadays when his predominant emotion appears often to be mathematical aestheticism. And so he bestrides the field of physics, if not like a Colossus, at least like a Plato hypostatising his numbers. It is high time that the practical physicist should make it clear that he is in charge of the 'scientific scrutiny.'

Now this laboratory-work is on the level of ordinary common sense, it is practical and pragmatic. It makes no difference whether the operator is a materialist or an idealist. In the study, in the church, in the ballot-booth, physicists differ as do other men; but they agree in the laboratory, at least they agree in the type of reasoning and testing which is final. They concur in what the number 'length' signifies, though they may differ as to whether our idea of space is innate or acquired, as to how our sensations

[69] *Nature of the Physical World,* 1928, p. 109.

[70] *Science and the Unseen World,* 1930, p. 22.

of touch and sight are correlated, and so on. Physics does not rise, and cannot rise, above the laboratory level of experience. Philosophy professes to do so; it therefore goes beyond the science of physics.

(2) The theoretical physicist, on the other hand, operates only with number-symbols; his task is to secure various algebraic relationships. His data come from the laboratory, and his conclusions must go back there; meanwhile he is a free man. He usually indeed clothes his operations in concrete phraseology; he uses terms which suggest that he is engaged in tasks pertaining to ordinary life or to the instrument-bench. But this, while psychologically understandable, is logically irrelevant. He must, of course, remember that he is not engaged in abstract algebra, that he is working towards a combination of ratios verifiable mensurationally by his practical colleague. Hence all his general equations must consist of tautometric functions. Usually too some or all of his symbols represent basic or derived quantities; his x, y, z stand for lengths measured in perpendicular directions, his t signifies duration or date. But it is quite legitimate for him to disappear for a time from his astonished colleague's gaze, to plunge into analytical metaphors—to work with four, five, six or more 'dimensions,' to invent potential-waves or even probability-waves. Provided that, at the end of this interim theorising, he re-emerges with something the ordinary man can 'bite,' some result that the lab-man can test.

This skilful and highly imaginative manipulation of numbers, this interim algebraising, is not 'physics' in the sense that it informs us concerning 'the nature of the physical world' or tells us about 'the mysterious universe.' It is a mathematical interval between two sets of experiments; it is an 'as if,' one of the many possible as if's; and in many cases it merely cloaks our ignorance. It is justifiable—at least as a *pis aller*—if it feeds verifiable results into the experimenter. We may talk as much as we like about the moving observer, space-time and matrices—provided we do not talk nonsense, as is often done. But our results have to be tested by a man using the Space and Time of ordinary life, by the observer in the laboratory.

Theorists may well become 'convinced of the formal and symbolic character of the entities of physics.'[71] It is quite natural, since they are working with pure numbers, and nowadays

[71] Eddington, *Nature of the Physical World*, 1928, p. 280.

they are given a free hand. But they must stop their formalism when they get back to earth. They cannot then say [72] that ‘the inertia or mass which makes the object difficult to move is symbolised by *x*,’ i.e. by *m*, the mass. For one thing, the mass cannot ‘symbolise’ itself. (No apology is needed for insisting on precision of language, when a new philosophy is being built upon it.) By the time the theorist has returned to the difficulty of moving things, he is talking the language of ordinary life. And now there is nothing symbolic or formal about the number *m*; it is a ratio found in the laboratory. Neither is there anything mysterious about its ‘background,’ which is to be discovered in the balances, inclined planes and what-not scattered about. Furthermore, the theorist is not entitled to saw off the tree-branch on which he is sitting. It will not do to say [73] that ‘matter may be defined as the embodiment of three related physical quantities: mass (or energy), momentum and stress.’ For *matter* is something with which we deal in ordinary life and in the laboratory; whereas the theorist is dealing with algebra. And, when you come to think of it, is it not silly to tell us that matter, so concretely evident in our experience, is the ‘embodiment’—i.e. the corporealisation or materialisation—of three numbers? When one retains a sense of reality, these theorists are not nearly so convincing as they are learned.

(3) We next come to ‘scientific concepts,’ which play such a large part in popular scientific philosophy. What are they? We are often told that they are *words* or symbols.

> A concept is a word denoting an idea which depends for its meaning or significance on the truth of some law. . . . Most, if not all, of the recognised laws of physics state relations between concepts [i.e. words].—N. Campbell, vii. 45.
>
> In natural science certain words have assumed a specific meaning. These [i.e. the words] may be called the scientific concepts which are the basis of all discussions and calculations. A physical law expresses an accurate numerical relation between such concepts.—F. A. Lindemann, *The Physical Significance of the Quantum Theory*, 1932, p. 14.
>
> A symbol may be defined as a mark of characteristic shape, which is taken to represent a certain idea or group of ideas. . . . Among the properties of relation which may be assigned to symbols [i.e. marks or shapes] are those of equality and order.—R. Lindsay and H. Margenau, *Foundations of Physics*, 1936, pp. 6, 8.

[72] Eddington, *New Pathways in Science*, 1935, p. 293.
[73] Eddington, *Nature of the Physical World*, 1928, p. 262.

We shall not waste time refuting this crude nominalism, which would reduce physics to playing with words, asterisks and blots. It is hardly meant seriously; probably it is mere looseness of language. Let us look rather to the applications. The last-quoted authors tell us (pp. 1, 3) that 'the physicist constructs what he terms the physical world, a concept which arises from a peculiar combination of observed facts and the reasoning provoked by their perception'; but this concept 'is not to be construed as being identical with the real world.' Take pressure as an example. This is 'a new quantity' (p. 12), a "symbolic concept' (p. 23): 'The whole matter is summed up in the one phrase: measurement of p.' Later (p. 21) we meet with 'the definition of a new concept, that of *mass*, with a symbol to represent it.' As for electricity, Prof. Bragg tells us (p. xi) that 'technical terms . . . cannot be avoided in a subject like Electricity, which bristles with new conceptions.'

Perhaps, once more, the difficulty is one of terminology; but we think there is much more in it. Logically, in view of the foregoing exposition, we have to deny that there are any scientific concepts, i.e. specific meanings not occurring in ordinary experience. One does not need to study physics to learn the meaning of Length, Duration (and perhaps Force). The basic measures from which physics starts presuppose these concepts. The rest consists of derived quantities, i.e. numbers formed from algebraic combinations of the basic ratios. The absence of new concepts does not mean that physics is easy or that it lacks technical terms. As the history of physics shows, it is extremely difficult to hit upon the right combinations of numbers so as to satisfy nature's behaviour; and it is only very gradually that the necessary combinatorial analysis (i.e. pure mathematics) became sufficiently developed. There are technical terms in profusion, this book is full of them. But they are mathematical terms, collocations and operations applied to the right combinations of basic measures. Take 'mass' for instance; there is no new idea involved; it is just a number which luckily and ingeniously helps to correlate our observations. Pressure involves nothing but Force-ratio and Area-ratio; it is not a concept at all, nor is it 'symbolic'; it is a number.

Obviously there are ontological assumptions. The electron theory, for example, presupposes discontinuity; a current is regarded as composed of entities that move. But we are quite

familiar in everyday life with things that move ; there is no new concept here. As for ' charge ' itself, it is a measure defined in terms of force and distance. When Dr. N. Campbell (vii. 105 f. instances ' length, weight, period, electric current, voltage, . . . temperature ' as new scientific concepts, he is merely cataloguing measure-numbers. They do indeed presuppose a long and laborious search, an elaboration based on careful experimentation. Nevertheless they do not contain a single new *idea* not already familiar to us in the world of common experience. Hence we cannot admit the following complaint :

> Such concepts as those of electricity, magnetic force and quanta of energy strain our imaginative capacity to its utmost limit in the attempt to conceive of them in terms of the objects we know. Conceived indeed as physical objects they are the merest ghosts, retaining only spectral vestiges of a very few of those qualities in virtue of which a so-called physical object lives and has its being.—Joad, *Philosophical Aspects of Modern Science*, 1932, p. 177.

These numbers are not concepts, they are not physical objects, they are not ghosts ; they happen to be measures. Every one of them is defined in terms of familiar concepts. The fact that these numbers are connected by certain mathematical relations, may excite our astonishment ; but there is no reason why it should strain our imaginative capacity.

It is true, however, that there is an almost irresistible tendency towards the reification of these numbers, which a distinguished critical historian of physics thus seeks to palliate :

> Let us now recall what we stated about scientific concepts . . . such as mass, force, energy. These concepts in the beginning are evidently only relations. Mass is the coefficient which bodies manifest at the moment of mechanical action ; force is only the cause of the acceleration, which is a difference of two velocities ; energy is a concept still more complicated, impossible in certain cases to define in its entirety. . . . This does not prevent physics from manifesting the tendency to treat these concepts as real things. In certain respects the reality which is attributed to them is even superior to that which common sense assumes in objects created by it.—E. Meyerson, *Identity and Reality*, 1930, p. 372.

We must first correct the terminology. Instead of ' concepts ' we must speak of ' numbers ' ; for ' relations ' we must substitute ' ratios ' ; and we cannot possibly say that one number is ' the cause ' of another. We start with numbers or ratios. This does not prevent physicists—not ' physics '—from treating these

numbers as 'real things,' more real even than the ordinary world from which we derived them. Similarly we were all brought up to believe in the stark reality of lines of force; and every electrical engineer has as vivid a picture of 'magnetic flux' as if it consisted of strings or emanated from a water-hose. This is quite understandable too; it eases the mathematical strain; often it ekes out the proof. But the psychology of physicists does not involve the logic of physics. Consider an example:

> The Conservation of Energy gives the relation: $mv^2/2 = wh$. Here both sides express *real things*; $mv^2/2$ is the kinetic energy acquired, wh the work expended in producing it. But if we choose to divide both sides of the equation by $v/2$ (the average velocity during the fall), we have by a perfectly legitimate operation: $mv = wt$, where t is the time of falling. . . . Here, although the equation is strictly correct, it is an equation between purely artificial or non-physical quantities, each as unreal as is the product of a quart into an acre. It is often mathematically convenient, but that is all.—P. G. Tait, *Life and Scientific Work*, 1911, p. 287.

Most people do in fact believe in the almost-substantial existence of 'energy.' This does not mean that the *number* ($mv^2/2$ or whatever it may be) is hypostatised; it means that this number is correlated with some real entity. This may be true, though people are not quite so sure as they once were; but it is a pious belief extraneous to physics, though exceedingly useful as a mnemonic. It is however quite unjustifiable to designate one equation as containing 'real things,' and to brand the same equation, with a factor removed, as connecting artificial non-physical quantities; for the equation contains nothing but algebraic numbers. The product mv is comparable, not to quart $\times$ acre, but to 3×7.

By way of contrast with Tait's contempt for momentum and worship of energy, let us quote Eddington's contrary view:

> If it is objected that they [mass and energy] ought not to be confused, inasmuch as they are distinct properties, it must be pointed out that they are not sense-properties but mathematical terms expressing the dividend and product of more immediately apprehensible properties, viz. momentum and velocity. They are essentially mathematical compositions and are at the disposal of the mathematician.—Eddington, *Space Time and Gravitation*, 1920, p. 146.

The plea that m and $mv^2/2$—or even $mc^2(1 - v^2/c^2)^{-\frac{1}{2}}$—can be confused because they are mathematical terms and compositions,

or, in plain words, numbers, is surely untenable. Equally erroneous is the assertion that these other numbers mv and m are immediately apprehensible properties; also the view that we can divide and multiply apprehensible properties. Moreover, the quotation just given from another book of Eddington's seems difficult to reconcile with his *Nature of the Physical World*.

It is easy enough to deal with Tait, for, though as a philosopher (good or bad) he passes a derogatory judgement on the equation $mv = wt$, he admits that it is perfectly legitimate and valid in physics. But it is not so easy to deal with Eddington who professes to base his philosophy on the 'remorseless logic' of physics. We have heard him declaring that physics consists of 'a scheme of symbols connected by mathematical equations.' Now mathematical equations contain nothing but numbers and numerical operators. So this declaration is identical with our thesis that theoretical physics operates with numbers. As to 'what the symbols stand for'—more accurately, as to what these numbers signify—physics, according to Eddington, is 'indifferent.' That is, during the theorist's interim period of mathematisation. But physics requires that when the theorist comes to the laboratory for confirmation, he must produce numbers which are ratios or measures. It is therefore with considerable surprise that we read the following pronouncement which is now rather famous:

> I have settled down to the task of writing these lectures and have drawn up my chairs to my two tables. Two tables! Yes; there are duplicates of every object about me. . . . One of them has been familiar to me from earliest years. It is a commonplace object of that environment which I call the world. . . . Table No. 2 is my scientific table. It is a more recent acquaintance and I do not feel so familiar with it. It does not belong to the world previously mentioned. . . . My scientific table is mostly emptiness. . . . It supports my writing-paper as satisfactorily as table No. 1; for when I lay the paper on it, the little electric particles with their headlong speed keep on hitting the underside, so that the paper is maintained in shuttlecock fashion at a nearly steady level. . . . There is nothing *substantial* about my second table. . . . I need not tell you that modern physics has by delicate test and remorseless logic assured me that my second [or] scientific table is the only one which is really there—wherever 'there' may be. . . . We must bid good-bye to it [the first table] for the present, for we are about to turn from the familiar world to the scientific world revealed by physics.—Eddington, *Nature of the Physical World*, 1928, pp. xi–xiv.

Let us try to summarise.

(1) There are two of everything : two tables, two laboratory-benches, two Eddingtons—or at least he has two organisms.

(2) The first, which is ' visible to my eyes and tangible to my grasp '—i.e. to my non-scientific eyes and to my ordinary grasp—is a ' strange compound of external nature, mental imagery and inherited prejudice.' This of course is a severe criticism of the practical physicist and of everything in his laboratory. When we come to physics—theoretical, of course—we must ' bid good-bye ' to all that.

(3) The second, to which we come after this farewell, is ' scientific,' it belongs to another world—' the scientific world revealed by physics.' It is in fact, as we have just seen, ' a scheme of symbols [numbers] connected by mathematical equations.'

(4) But, curiously enough, this deutero-table, which is only an equation, begins to behave in a way which seems strangely familiar to the unregenerate non-physicist. I can ' draw up ' a chair to it, and can move it to a chair. It ' supports ' objects. It contains ' particles ' which have ' speed ' and ' keep on hitting ' other things. There is nothing ' substantial ' about it, indeed ; but this seems merely to mean that ' its substance (if any) [is] thinly scattered in specks in a region mostly empty.' And it is localised, it is ' really there,' or at least the specks are.

(5) In fact it is ' the only one which is really there.' The experimentalist ' by delicate test '—i.e. by measuring ordinary lengths, times, etc., in the laboratory—has succeeded in getting rid altogether of the laboratory as we know it from experience, as it is described in architects' plans and in instrument-makers' catalogues. If we feel the ' good-bye ' involved, we can console ourselves with the reflection that we are obeying ' remorseless logic.'

This, we reiterate, may be philosophy, good or bad. But it should not be propounded to us as physics, as a view to which all physicists are committed. The object of science is to provide correlation and control for the events in the workaday world, not to set up a weird and ghostly counterfeit. The mathematical excursions of the theorist are legitimate only if they lead to some new correlation of measurable quantities which can be tested in the laboratory. Apart from such a mathematical calculus as has been developed for the quantum theory, the ordinary theories

of physics are comprehensible only if taken in conjunction with the implied background of laboratory space and time. The electron theory, for instance, is inevitably bound up with an ontological assumption, viz. a discontinuous spatial distribution of charge. The atomic and intra-atomic theories imply a similar discontinuity of matter. There is nothing peculiarly revolutionary in such an idea, which has been familiar since the time of Democritus. There is a refinement of sensation here, an extrapolation beyond its range ; but no contradiction ; above all, no duplication. Our everyday sense-experience does not tell us of the existence of mathematical continuity, but only of visual and tactual continuity—which still remain as stubborn facts. What physics tells us about the chair does not contradict what ordinary experience tells us ; it supplements and extends that knowledge. A sharp razor-blade seen under a powerful microscope displays a serrated and jagged edge. But this does not entitle us to proclaim to the world that there are two razors, the 'real' one being a murderous-looking micro-weapon. As a philosopher, Sir Arthur Eddington may, so far as this book is concerned, pin his faith to, or write his Gifford Lectures upon, the 'scientific table.' But when he tries to argue that every student and teacher of physics is obliged to adopt the same creed, his arguments, like his table, are 'mostly emptiness.'

10. Coincidences.

The common-sense thesis defended in this chapter may be thus summarised : (1) The theoretical physicist is dealing with pure numbers. (2) In so far as these numbers and their algebraic relationships pertain to physics—that is, in so far as they originate in the laboratory or are brought thereto for confirmation or rejection—they are measures or ratios or relationships between them. (3) These measures are ascertained in the laboratory at the ordinary pragmatic or operational level of experience, without any reference to abstruse questions of psychology, epistemology or ontology. Against Sir Arthur Eddington we objected under each of these headings : (1) His use of the word 'symbols,' instead of algebraic numbers and mathematical operations thereon, is very misleading and is calculated to involve irrelevant philosophical implications concerning the 'symbolic' nature of knowledge. And the reification of these 'symbols' into

'scientific objects' leads to a curious modern revival of the views of Pythagoras and Plato. (2) The theorist is inclined to forget that his ultimate tribunal is the laboratory. This is the only 'background' to which, as a physicist, he has the right to refer and the duty to defer. (3) A philosophy cannot be constructed from physics; when a man who happens also to be a physicist writes meta-physics, he is advocating views which lie outside the domain of scientific physics.

We must now deal with another distortion or exaggeration, which the theory of 'relativity' has made widespread. This goes to the other extreme. So far from ignoring the laboratory-man, this new view glorifies and exalts him, or rather his instruments. No more talk now of mysterious symbols; the numbers occurring in physics are just pointer-readings, coincidences of a needle-point with a mark on a dial—and they are nothing more. We shall take Eddington as the most brilliant exponent of this new theory also.

> Let us examine the kind of knowledge which is handled by exact science. If we search the examination papers in physics and natural philosophy for the more intelligible questions, we may come across one beginning like this: 'An elephant slides down a grassy hillside. . . .' . . . He reads on: 'The mass [weight] of the elephant is two tons.' Now we are getting down to business; the elephant fades out of the problem and a mass [weight] of two tons takes its place. . . . Two tons *is* the reading of the pointer when the elephant was placed on a weighing-machine. Let us pass on. 'The slope of the hill is 60°.' Now the hillside fades out of the problem and an angle of 60° takes its place; . . . 60° *is* the reading of a plumb-line against the divisions of a protractor. Similarly for the other data of the problem. The softly yielding turf on which the elephant slid is replaced by a coefficient of friction, which though perhaps not directly a pointer-reading is of kindred nature. . . . By the time the serious application of exact science begins we are left with only pointer-readings. . . . It was not the pointer-reading of the weighing-machine that slid down the hill! And yet from the point of view of exact science the thing that really did descend the hill can only be described as a bundle of pointer-readings. It should be remembered that the hill also has been replaced by pointer-readings, and the sliding down is no longer an active adventure but a functional relation of space and time measures. . . . All the characteristics of the elephant are exhausted and it has become reduced to a schedule of measures. There is always the triple correspondence: (*a*) a mental image which is in our minds and not in the external world; (*b*) some kind of counterpart in the external world, which is of inscrutable nature; (*c*) a set

of pointer-readings which exact science can study and connect with other pointer-readings. . . . *It is this connectivity of pointer-readings, expressed by physical laws, which supplies the continuous background that any realistic problem demands.*—Eddington, *Nature of the Physical World*, 1928, pp. 251–255.

The view could not be expressed more clearly. Inasmuch as it is a subtly humorous distortion of the thesis we have been maintaining, it will be advisable to investigate how exactly Eddington succeeded in persuading himself that scientifically—presumably in physics but not in zoology—an elephant is a bundle of pointer-readings. We therefore insert another quotation from the same passage.

I should like to make it clear that the limitation of the scope of physics to pointer-readings and the like, is not a philosophical craze of my own but is essentially the current scientific doctrine. It is the outcome of a tendency discernible far back in the last century, but only formulated comprehensively with the advent of the relativity theory. The vocabulary of the physicist comprises a number of words such as length, angle, velocity, force, potential, current, etc., which we call 'physical quantities.' It is now recognised as essential that these should be *defined* according to the way in which we actually recognise them when confronted with them, and not according to the metaphysical significance which we may have anticipated for them. In the old text-books mass was defined as 'quantity of matter'; but when it came to an actual determination of mass, an experimental method was prescribed which had no bearing on this definition. . . . Einstein's theory makes a clean sweep of these pious opinions, and insists that each physical quantity should be defined as the result of certain operations of measurement and calculation.—*Ibid.*, p. 254.

And now we begin to understand and even to sympathise. Eddington is in righteous reaction against such descriptions of the number m as 'quantity of matter' and that misinterpretation of 'physical quantities' which has been largely due to Maxwell's influence. So far we are in agreement, though he is a little vague when he says that physical quantities (i.e. numbers) should be defined according to the way in which we recognise them when confronted with them (i.e. these numbers). Why not say simply that they are ratios or measures? But Eddington is historically incorrect when he states that this view is due to Einstein's theory. There is no connection whatever; and the current pronouncements on 'space-time' and 'relativist units' show that erroneous 'pious opinions' are still prevalent among

Einstein's followers. Apart from this, Eddington's statement that 'the limitation of the scope of physics to pointer-readings and the like . . . is essentially the current scientific doctrine,' coincides with the view maintained here, provided we insert two amendments in order to prevent misunderstanding.

(1) For 'physics' read '*theoretical* physics.' For obviously the task of the lab-man is not primarily to investigate the connectivity of algebraic numbers, typified by x, his job is to *produce* these numbers by the typical operation $x = A/B$. (2) For 'pointer-readings and the like' read 'ratios or measures.' For clearly the measuring-mechanisms typified somewhat crudely by pointer-and-dial are very diverse; some register weight, some indicate voltage, others measure length, and so on. Each has been designed, with the aid of practical manipulation and elaborate argument, to enable the investigator to find some definite ratio, simple or compound. It is true that all these ratios are numbers, but their physical context is entirely different. We can no more jumble them all together under the common denomination of 'pointer-reading,' than we can confuse the numbers l, m, t, v, j. Of course, *quâ* numbers they cannot be discriminated; and if we were engaged in pure mathematics, that would be the end of the story. To call l, m, t, etc., 'pointer-readings' is as true as to call them 'ratios.' But it is ludicrously false to infer from this that we are not interested in *what* they are ratios of, in what information each 'pointer' is designed to convey. It is as if a man entered a shop and asked for '1 lb.,' with supreme indifference as to whether it was tea or butter; or as if a person, imbued with 'the current scientific doctrine,' insisted that x yards of cloth were identical with x grams of acid.

Let us now revert to Eddington's parable or parody. Needless to say, examination papers in mechanics deal with a rigid body sliding down an inclined plane, but not with an elephant on a grassy hillside. That is Sir Arthur's little joke; this white elephant is the second jocular beast encountered in the dry subject of mechanics; the first was a monkey climbing up a rope, invented by Todhunter or some other facetious don. When the freshman gets down to business, when he makes this serious application of exact science, or in plain English, when he solves this elementary question, he obtains something like this:

$$f = g(\sin \alpha - \mu \cos \alpha).$$

He is 'left with only pointer-readings,' or—as we have been explaining *ad nauseam*—he has an equation between numbers. One wonders if Eddington expected the equation to include the elephant and the hill! Were it not for the fact that this little story occurs in a serious philosophical work and is made to sustain a metaphysical thesis, one would be inclined to take it as a practical joke. For we are seriously told that the elephant is exhausted of his characteristics, becomes a sliding bundle of pointer-readings, and is reduced to a schedule of measures. What a danger to life is involved in using a weighing-machine and a plumb-line! If this be 'the point of view of exact science,' it is indistinguishable from nonsense.

The services of the elephant are enlisted not to amuse juvenile readers but to impress on serious philosophers that there is a triple correspondence: (*a*) a mental image, (*b*) some kind of inscrutable counterpart like the Kantian thing-in-itself, (*c*) pointer-readings connected by physical laws, i.e. numbers connected by equations. Consideration of (*a*) and (*b*) is beyond the scope of a book on physics. We therefore limit ourselves to examining (*c*), those pointer-readings with which alone 'exact science' can deal, whose algebraic interrelations supply 'the continuous background' for our realistic aspirations. As the following quotations show, this thesis is a pet assertion of relativists.

> Such encounters constitute the only actual evidence of a time-space nature with which we meet in physical statements. . . . Every physical description resolves itself into a number of statements, each of which refers to the space-time coincidence of two events.—Einstein, *Relativity*, 1920, p. 95.
>
> All our physical experience can be ultimately reduced to such coincidences [of point-events].—Einstein, in *The Principle of Relativity*, 1923, p. 118.
>
> As emphasised by Einstein, every observation or measurement ultimately rests on the coincidence of two independent events at the same space-time point.—Bohr, *Nature* 121 (1928) 580.
>
> A critical review of exact science teaches us that all our observations resolve finally into such coincidences. . . . All that is actually observable consists of space-time coincidences.—Born, *Einstein's Theory of Relativity*, 1924, p. 266 f.
>
> Everything else in our world-picture which cannot be reduced to such coincidences is devoid of physical objectivity and may just as well be replaced by something else.—Schlick, *Space and Time in Contemporary Physics*, 1920, p. 50.

Nobody ever observes anything but the coincidence of two marks.—Swann, *The Architecture of the Universe*, 1934, p. 301.

Against this fashionable and orthodox thesis, we shall now submit some arguments.

If the statement were limited to that very modern specialisation within the ordinary observational world, which is exemplified in an up-to-date physical laboratory, it would be difficult to accept. But it passes the bounds of credulity to apply it to 'all our physical experience,' to say that 'nobody ever observes anything but the coincidence of two marks.' Sensations of heat and cold, experience of resistance, estimates of duration, appreciation of musical intervals, vision of shapes and colours—they are all nothing but coincidences of pairs of marks. It would almost seem as if people trained to watch the motion of a light-spot over a scale or the reading of a pressure-gauge became subject to hallucination. Even the very enunciation of the proposition displays its falseness. For we cannot have pointer-readings without a pointer and a dial; and whatever these latter may be, they certainly are not 'readings' or numbers. Two marks cannot coincide unless we start with marks recognisable when non-coincident; and whatever these marks are, they are not a coincidence. So, however homogeneous and alike these readings may be when regarded as mere numbers, they are not self-sufficient or self-explanatory; they presuppose a piece of apparatus, some non-numerical physical entity. When we say that an elephant weighs two tons, we imply an elephant, an object labelled '2 tons' and a machine. To say that exact science reduces the elephant to the number *two*, is either to talk nonsense or else to express the theoretical purist's disdain for anything but his x's and y's.

Moreover, a measuring mechanism is an elaborate technical device. We are not content with any reading from any pointer. We say this pointer reads lbs. and that pointer reads watts. Mere coincidences, mere readings, are of no use to anyone. Dr. N. Campbell (iv. 40), who is a laboratory worker as well as a relativist, tells us that some writers suggest

> that all the phenomena studied in physics can actually be reduced to coincidences of moving points. They draw attention, for example, to the use of a coincidence between a spot of light and the divisions of a graduated scale to measure many magnitudes, e.g. electric currents. But of course such suggestions are absolutely

> false and would not appeal to anyone actually familiar with experiment. The coincidence of the galvanometer spot with the division on the scale measures a current only if many conditions are fulfilled, if the instrument is constructed in a certain manner and if it is connected to the rest of the circuit in a certain way. And these conditions cannot be described in terms of coincidences of points; no coincidence would be altered if we replaced iron by wood and copper by glass; but the deflection of the galvanometer would be very greatly affected by the change.

If certain conditions are fulfilled, we regard the deflection of a light-spot as measuring 'current'; in other cases we take it as measuring a torque or an elongation. The same coincidence may have very different meanings in different physical contexts. In fact our recourse to coincidence at all is, strictly speaking, accidental; it allows a more accurate judgement than is attainable by a direct comparison. It depends on the fact that we can make variations of objective circumstances correspond to certain changes of position; and these latter—thanks to 'rigid' bodies susceptible of graduation and to acuity of sight aided by optics—are capable of being very accurately estimated. But in reality there is no equality entirely reducible to congruence.[74] We should be obliged to abandon the criterion of congruence (indiscernibility by coincidence), if the congruence of the positions of the thermometer-column did not correspond to equal heat-sensations, if congruent inclinations of the balance did not correspond to other sensible effects of weight. And there are cases in which equality is estimated without spatial coincidences, e.g. equality of illumination of the two halves of a photometer-disc, the equality of saturation of two tints, the equality of the pitch of two directly perceived sounds. As Bridgman says (i. 167):

> How, for example, shall we describe in terms of space-time coincidence the photometric comparison of the intensity of two sources of illumination, or the comparison of the pitch of two sounds, or the location of a sound by the binaural effect? To justify the coincidence point of view, we apparently have to analyse down to the colourless elements beyond our sense-perception.

It is difficult to know what is meant by the enigmatic apology in the last sentence; it seems to mean that the pointer-readings are not readable—by us!

[74] Cf. A. Spaier, *La pensée et la quantité*, 1927, p. 217.

Two more, among many other possible, objections may be mentioned. The reduction of practical physics to a number of undifferentiated pointer-readings displays a curious monadic or atomistic view of nature. Obviously distance is reduced to position and duration to date. One pointer-reading can give only position ; to obtain distance we must correlate two pointer-readings by a spatial operation which goes beyond the mere algebraic accumulation of 'readings.' The case is still clearer as regards duration and motion. Says Einstein :

> When we were describing the motion of a material point relative to a body of reference, we stated nothing more than the encounters of this point with particular points of the reference-body. We can also determine the corresponding values of the time by the observation of encounters of the body with clocks, in conjunction with the observation of the encounters of the hands of clocks with particular points on the dials.—Einstein, *Relativity*, 1920, p. 95.

Here, in order to atomise time, things called point-clocks are invented. They have then to be 'synchronised' by light-signals, i.e. by means of a physical process which expressly contradicts the thesis that the scientific world is an aggregate of monadic point-events. Thus the dates of the point-clocks are changed so as to register the duration of a motion. And how, in this theory, is motion to be accommodated at all ? Eddington is a bit squeamish ; he does not like saying that the pointer-reading of the weighing-machine slides down the hill. 'And yet,' he thinks, 'from the point of view of exact science the thing that really did descend the hill can only be described as a bundle of pointer-readings.' So pointer-readings can be collocated in a bundle to form a thing ; and this thing can descend a hill. The hill is not a pointer-reading, nor is the action of descending. Positions and dates are obviously deficient ; so distances and durations are quietly smuggled in. Whether this is done by a light-beam encountering clocks adjusted by point-attendants, or by boldly assuming the descent of a bundle, matters little. The monadism of point-events or pointer-readings has to be shattered to accommodate experience.

And finally, we may ask without trespassing on the theory of relativity, suppose there are no point-clocks ? After all, these convenient little mechanisms are only a myth, a supposition on paper. And when an elephant slides down a grassy hillside, it is most unusual to weigh him accurately just before his adventure ;

the coefficient of friction between elephant-hide and grass is a problematical quantity; and surveyors have not measured the gradient of every hillside. Or, to express our objection seriously, what happens when measures are not actually carried out, when there are no pointer-readings? The world does not wait for 'exact science.' The tides rose and fell before Kelvin invented his analyser; a physicist eats his chop even if the butcher's pointer-reading was inaccurate; light keeps coming to us from the sun even though it encounters no clocks *en route.*

If anyone thinks this argument undignified or irrelevant, let him re-read Eddington's elephant-story plus its philosophical conclusions, and then let him read the following quotation:

> Now we realise that science has nothing to say to the intrinsic nature of the atom. The physical atom is, like everything else in physics, a schedule of pointer-readings. The schedule is, we agree, attached to some unknown background. . . . We have dismissed all preconception as to the background of our pointer-readings, and for the most part we can discover nothing as to its nature. But in one case—namely, for the pointer-readings of my own brain—I have an insight which is not limited to the evidence of the pointer-readings. That insight shows that they are attached to a background of consciousness. . . . If we must embed our schedule of indicator readings in some sort of background, at least let us accept the only hint we have received as to the significance of the background—namely, that it has a nature capable of manifesting itself as mental activity.—Eddington, *Nature of the Physical World*, 1928, p. 259.

Everything in physics is a schedule or bundle of pointer-readings. Now one of two things. (1) The setting up of the recording mechanism, the graduation of the dial, the reading of the pointer's position by an observer, are all essential. It follows that at least *some* things in physics—namely the pointers and the dials and so on—are not themselves pointer-readings. It also follows that, in the absence of pointers, etc., the atom, the brain, and so on, do not exist—at least for exact science. (2) Alternatively, all this talk of pointer-readings is irrelevant; these are merely practical expedients for approximately ascertaining pre-existent objective ratios. One Length or Duration would still be twice another, even though we had no dial on which to read the number two. In that case, everything in physics cannot be a schedule of ratios. For ratios are not scheduled or bundled in any spatial or temporal sense; and a ratio must be

the ratio of some one quantitative entity to another. The mass of a hydrogen atom (or molecule) is expressed as a small fraction of a gram ; it would be rather ridiculous to say that the piece of metal marked a gram *is* the number one and that the hydrogen atom *is* the number $1 \cdot 665 \times 10^{-23}$; and the ridiculousness would not be lessened by ' scheduling ' various such numbers together. What is euphemistically called modern physics starts with the statement that the mass of A is so many times the mass of B—the only common-sense assertion we can make about mass ; first B is dropped, so that now we have to believe that the mass of A is so many times ; then A is dropped, and we are left with so-many-times. It is as if we started with the dog wagging the tail (or perhaps vice versa) ; first the dog is dropped, then the tail ; so that finally we are left with ' wag '—background unknown.

It is when we come to the ' brain,' however, that we acquire insight into Eddington's philosophical purpose. Apparently I know (1) the pointer-readings of my own brain and (2) their attachment to a background of consciousness. Therefore all the pointer-readings of physics have a background of a mental nature. Mr. Joad expressed the obvious objection :

> The argument clearly implies that the physicist knows the pointer-readings of his own brain, or more precisely—since his brain is presumably only a schedule of pointer-readings—that he knows his own brain.—*Philosophical Aspects of Modern Science*, 1932, p. 40.

To this Sir Arthur Eddington replies as follows :

> Mr. Joad has not grasped what is implied by the symbolic character of physical entities. It is as though, having said, ' Let m be the mass,' I was supposed to be guilty of confusion in treating m both as an algebraic symbol and as a physical magnitude. . . . How can a bundle of pointer-readings start a mental process ? He might equally ask how can an algebraic symbol m make it difficult to shift an object ? The answer is that the inertia or mass which makes the object difficult to move is symbolised by m. And similarly the bundles of pointer-readings symbolise the processes which start the messages.—*New Pathways in Science*, 1935, p. 293 (substituting m for x).

Sir Arthur here shifts his position.

(1) He tacitly admits the irrelevance of the pointer and dial. ' Physical entities ' such as l, m, t are ' algebraic symbols,' as we have been contending.

(2) These numbers or ratios ' symbolise ' things called ' physical

magnitudes,' of which an example is inertia or mass (though currently in physics this term also denotes the number m). He therefore tacitly withdraws his assertion that everything in physics *is* a schedule of pointer-readings. What he now says is that the various physical magnitudes associated with objects—the physical atom, the brain, etc.—are symbolised by algebraic symbols, i.e. numbers or ratios. But he must now logically withdraw his former italicised contention that these numbers and their algebraic relationships supply 'the continuous background that any realistic problem demands.' Pure numbers, ratios, are hardly a realistic background.

(3) He makes no attempt to show how I know the l, m, t, etc., of my own brain, in any sense comparable to the way in which an anatomist can know the weight, etc., of a corpse's brain.

(4) He does not reconcile his two former assertions (a) that in general the 'background' consists of 'the connectivity of pointer-readings' and that for the particular case of my brain the pointer-readings are 'attached to a background of consciousness,' and (b) the obvious implication of the present passage that the pointer-readings now designated 'algebraic symbols' have 'physical magnitudes' as background.

(5) He continues to employ the rather mysterious term 'symbolise.' He can hardly mean it in the sense in which an 'algebraic symbol' symbolises a number, i.e. it is a letter such as x whose meaning is an unspecified number. Presumably l symbolises Length as m symbolises Inertia ; this is only another—and a rather ambiguous—way for saying that these letters stand for ratios ascertainable in a physical laboratory. But if this is so, the symbolisation is entirely within the laboratory and within the range of ordinary operational life. The numbers are not ratios of mysterious entities, they are the ratios of Space and Time as understood by the ordinary run of men ; their 'background' is daily life.

(6) Having now brought physics back to the pragmatic activities of the laboratory, we can see at once how impossible it is that any juggling with the symbols of physics could throw any light on philosophy or psychology. Eddington does not like the question : How can a bundle of pointer-readings start a mental process ? His answer is this : Just as m symbolises Inertia, 'the bundles of pointer-readings symbolise the processes which start the messages.' That is, just as the number m is the measure

called mass, so the other numbers—if we could ascertain them—would be the measures of the physical processes (or rather of 'physical magnitudes' connected with them) which start mental processes. It seems rather an anti-climax! The suggestion about the mental character of the background of nature seems also to disappear. For now it can be boiled down to this: My brain has physical properties which could, theoretically at any rate, be measured by a physicist or preferably by a physiologist. These properties are in some mysterious way known to be correlated or associated with my consciousness. But other things—atoms, bricks, water, etc.—also have measurable properties. Therefore they too are associated with consciousness—not mine or yours, of course; they have a background capable of, though not actually, manifesting itself as the mental activity of nobody in particular. The conclusion may or may not be true, we are not investigating it. But the argument, which does not transcend a schoolboy's knowledge of physics, is neither very original nor very cogent.

CHAPTER XV

UNITS AND 'DIMENSIONS'

1. Electrical 'Dimensions.'

We are now in a position to refute most of the accepted statements concerning electromagnetic 'dimensions' and to contrast our common-sense treatment with the peculiar and really unintelligible accounts with which our text-books provide us.

Putting $Q = JT$ in the first and last equation of (14.11), we have

$$M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-2}[\alpha^{\frac{1}{2}}] = J = M^{\frac{1}{2}}L^{\frac{1}{2}}T^{-1}[a\beta^{-\frac{1}{2}}]. \qquad (15.1)$$

Whence, as before, we at once obtain (14.12)

$$[a/\alpha^{\frac{1}{2}}\beta^{\frac{1}{2}}] = L/T. \qquad (15.2)$$

Let us now see how the text-books interpret and treat this equation. The symbols, of course, are not regarded as numbers at all; apparently they are essences or quiddities, mysterious entities which, rather surprisingly, are amenable to the ordinary arithmetical operations (multiplication, division, etc.)—extraction of the square root being, as we have seen, a sore point. The textbooks also invariably take $[a] = 1$. A more serious error is the universal confusion of α and β with $\varkappa$ and μ.

The first method of dealing with (15.1) is illustrated by the following quotation:

> If, instead of ignoring the dimensions of $\varkappa$ and μ, we keep these symbols, unknown as they are, in our definitions, we may assume that the same quantity however defined will possess the same dimensions. Taking for instance the two definitions of unit current, we have
>
> $$M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-2}[\varkappa]^{\frac{1}{2}} = M^{\frac{1}{2}}L^{\frac{1}{2}}T^{-1}[\mu]^{-\frac{1}{2}}.$$
>
> Thus
>
> $$[\varkappa]^{\frac{1}{2}}[\mu^{\frac{1}{2}}] = L^{-1}T.$$
>
> —W. C. Dampier Whetham, p. 181.

On our simple view, this is merely a reproduction of (15.1) and (15.2) with $[a] = 1$. The 'definition of unit current' is merely

the measure-ratio of current, and the 'unknown dimensions' of $\varkappa$ and μ are merely the arbitrary measure-ratios of α and β. But the writer fancies that he is equating (or identifying) the quiddities of the two unit currents, elm and mag : $J_1 = J_2$.

Other writers however give a different account. Thus a well-known text-book, after giving the above demonstration, adds :

> If we are using the dimensional equations simply to deduce the *dimensions* of any quantity expressed in the one system from its dimensions expressed in the other system, then the above relation is sufficient. If however we require to find the *numerical equivalent* for an electrical or magnetic quantity expressed in the one system as expressed in the other, we require to know the numerical value of the ratio $\varkappa^{-\frac{1}{2}}\mu^{-\frac{1}{2}}/LT^{-1}$.—W. Watson, *Text-book of Physics*, ed. Moss, 1920[7], p. 788.

That is, we are first given $[\varkappa\mu]^{-\frac{1}{2}} = L/T$, which is 'sufficient' for certain purposes. And then, for other purposes, we are given $[\varkappa\mu]^{-\frac{1}{2}} = cL/T$. It is rather confusing. Let us try again. Instead of equating units of current, let us get their ratio

$$J_1/J_2 = [\varkappa^{\frac{1}{2}}\mu^{\frac{1}{2}}]LT^{-1}.$$

Apparently it is allowed by the rules of the game to manipulate these symbols as if they were numbers. Anyway this equation is given by Guillaume,[1] who adds : 'The absolute dimensions of the ratio must be an abstract number ; hence we deduce

$$[1/\sqrt{\varkappa\mu}] = [LT^{-1}],$$

a fundamental relation between the magnitudes $[\mu]$ and $[\varkappa]$, whose absolute dimensions remain unknown.' Without pausing to puzzle out the absolute dimensions of the ratio of two absolute dimensions, we can at least point out that we started with $J_1/J_2 = 1/c$ and end with $J_1 = J_2$. It is curious to find this obvious contradiction left unexplained in the text-books. Starling (p. 390) tells us that 'it is unreasonable to suppose that one and the same quantity can have two different dimensions.' He then proceeds to equate the dimensions of elm and elst current, and deduces $[\varkappa^{\frac{1}{2}}\mu^{\frac{1}{2}}] = [L^{-1}T]$. But on the next page (p. 391*) he gives an entirely different account, which assumes that the dimensions are *not* equal :

> If j_1 be the number of electrostatic units in a given current, the complete expression for the current is $j_1 M^{\frac{1}{2}}L^{\frac{3}{2}}\varkappa^{\frac{1}{2}}/T^2$; and if j_2 be the number of electromagnetic units in the same current, $j_2 M^{\frac{1}{2}}L^{\frac{1}{2}}/T\mu^{\frac{1}{2}}$

[1] C. E. Guillaume, *Unités et étalons*, [1893], p. 30.

is its expression in electromagnetic measure, where j_1 and j_2 are mere numbers. Therefore

$$j_1 M^{\frac{1}{2}} L^{\frac{3}{2}} \varkappa^{\frac{1}{2}} / T^2 = j_2 M^{\frac{1}{2}} L^{\frac{1}{2}} / T \mu^{\frac{1}{2}},$$

or

$$[1/\varkappa^{\frac{1}{2}}\mu^{\frac{1}{2}}] = j_1/j_2 \,.\, [LT^{-1}]. \;.\;.\;.$$

We see then that $1/\sqrt{\varkappa\mu} = c$ cm. per sec., since $[LT^{-1}]$ is a velocity of one cm. per sec.

Now this last does not follow at all. For ostensibly it has been shown that c cm./sec. is equal, *not* to $1/\sqrt{\varkappa\mu}$, but to the *dimensions* of $1/\sqrt{\varkappa\mu}$. This difficulty also occurs in the treatment of Loeb (p. 70*), who deals with charge instead of current. Let there be q_1 elst units and q_2 elm units 'in a given quantity of electricity,' i.e. given ontologically not mensurationally. We must use 'the complete expression for the quantity.' Equating these complete expressions, we have

$$q_1 M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1} [\varkappa^{\frac{1}{2}}] = q_2 M^{\frac{1}{2}} L^{\frac{1}{2}} [\mu^{-\frac{1}{2}}]$$

in which Loeb has forgotten to fix the sacred square brackets on $\varkappa$ and μ. Here q_1 and q_2 'are mere numbers giving the numerical values involved,' and the equation just written is justified by the remark that 'the two expressions for quantity above represent the same quantity.' Hence

$$\frac{1}{c} = \frac{q_2}{q_1} = \frac{LT^{-1}}{[1/\sqrt{\varkappa\mu}]},$$

where we have again inserted the square bracket. But LT^{-1} '$= 1$ cm./sec.,' therefore $c = [\varkappa^{-\frac{1}{2}}\mu^{-\frac{1}{2}}]$; and not, as Loeb says, $c = 1/\sqrt{\varkappa\mu}$.

But if this mysterious thing called a 'dimension' can be an ordinary number like c, without even the '1 cm./sec.,' then the whole conception of qualitative designations is shattered. Let us see how they started. This is how Maxwell[2] introduced them (i. 46):

> We may now write the general law of electrical action in the simple form $F = ee'r^{-2}$. . . . If $[Q]$ is the concrete electrostatic unit of quantity itself, and e, e' the numerical values of particular

[2] The current version of the procedure is just as objectionable. 'If we make the charges equal and make f and r each unity, $q = q'$ becomes unity. . . . Putting 1 gm. cm./sec.^{2} for f and 1 cm. for r and solving for $q = q'$, we find the c.g.s. [*i.e.* elst] unit of charge as 1 gm.$^{\frac{1}{2}}$ cm.$^{\frac{3}{2}}$/sec.'—Page-Adams, p. 13. We first put $f = r = 1$ and $q' = q$, and naturally find $q = 1$. We next by some mysterious operation find $q = 1$ multiplied by a collection of hieroglyphics.

quantities; if $[L]$ is the unit of length and r the numerical value of the distance; and if $[F]$ is the unit of force and F the numerical value of the force, then the equation becomes

$$F[F] = ee'r^{-2}[Q^2][L^{-2}];$$

whence $$[Q] = [LF^{\frac{1}{2}}] = [L^{\frac{3}{2}}T^{-1}M^{\frac{1}{2}}].$$

We need not argue further against the nonsense of a unit of Length to the power $1 \cdot 5$. What we are interested in now is the genesis of these dimensions. We start with 'the general law' $f = qq'/r^2$. We then multiply one side by F and the other by Q^2/L^2, and then we equate these peculiar factors. The result is the 'electrostatic' dimensions of q. The procedure appears to be that we write a mysterious version of an ordinary numerical equation, entirely in qualitative symbols. If once we admit that these qualitative collocations can have purely numerical ratios, the whole idea from which we started is destroyed. Moreover the 'electrostatic' dimension is nowadays derived from the law $f = qq'/\varkappa r^2$, and the 'electromagnetic' dimension is derived from the incorrect law $f = mm'/\mu r^2$.

In order to find the origin of these misunderstandings, let us quote two well-known text-books with a slight change of notation.

> The force between them is proportional to qq'/r^2, a quantity of a fundamentally different kind. We therefore write $f = qq'/\alpha r^2$, and choose the dimensions of both sides of this equation the same. . . . In the simple electrostatic theory as we have developed it, we choose to measure a quantity of electricity so that the constant α in this expression is a simple number (without dimensions) numerically equal to unity.—Livens, i. 351 (with α written instead of $1/\gamma$).
>
> In the formula $f = qq'/\alpha r^2$ we can and do choose our unit of charge in such a way that the *numerical* value of α is unity, so that the numerical equation becomes $f = qq'/r^2$. But we must remember that the factor α still retains its physical dimensions. Electricity is something entirely apart from mass, length and time; and it follows that we ought to treat the dimensions of the equation by introducing a new unit of electricity Q and saying that $1/\alpha^{\frac{1}{2}}$ is of the dimensions of a force divided by Q^2/r^2 and therefore of dimensions $ML^3Q^{-2}T^{-2}$. If, however, we compare dimensions in the equation, neglecting to take account of the physical dimensions of this suppressed factor α, it appears as though a charge of electricity can be expressed in terms of the units of length, mass and time. . . . The apparent dimensions of a charge of electricity are now $M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1}$. It will be readily understood that these dimensions are merely apparent and not in any way real.—Jeans, p. 15 (with α substituted for $1/c^2$).

This is a typical specimen of the wretched metaphysics which is inflicted even on elementary students. After swotting up tables of dimensions, it is rather discouraging to be told that they are merely apparent and utterly unreal. It is a poor justification of electrostatics—and incidentally of astronomy—to say that we just choose to equate entities of fundamentally different kinds. As Helmholtz remarks (vi. 8), 'it has no real intrinsic meaning indeed, but we can usefully calculate with it'! The amount of perverted philosophy based on the simple equation $f = qq'/\alpha r^2$ —which a schoolboy could understand if he were left to himself—is really incredible. Here is another example:

> In our fundamental units this mechanical force [between electrified particles] is equal to a mass times a length divided by the square of a time. Now Coulomb . . . believed that electricity was a kind of fluid substance, . . . and with this idea in his mind, he employed the term quantity of electricity to indicate an analogy with a quantity of matter. On this supposition, a quantity of electricity expressed in mechanical units is equal to the square root of a length times a mass. Now it is quite certain that a quantity of electricity has nothing in common with length.—L. T. More, *The Limitations of Science*, 1915, p. 153.

Once more we encounter the fatal misunderstanding of scientific symbols. The number q, calculated from certain observed measures, i.e. from other numbers, is indeed called 'a quantity of electricity.' The phraseology may be unfortunate, but it cannot now be dislodged or amended. Outsiders may be pardoned for forgetting q and for being misled by the phrase which is technical. But surely we might expect professors of physics to understand their own calculus and vocabulary. To a man who has never carried out a measure in a laboratory, the statement that 'a quantity of electricity has nothing in common with length,' sounds pretty obvious and convincing, 'quite certain' in fact. Nevertheless it is just nonsense in physics, for it means that the number q has nothing in common with the number l! Moreover, having adopted this language, we have to be consistent. If we call q 'a quantity of electricity,' we cannot apply the same phrase to the ratio $Q = q_1/q_2$. Yet the equation $Q = L^{\frac{1}{2}}M^{\frac{1}{2}}$ (i.e. $[\alpha] = L^2/T^2$) is thus translated: 'a quantity of electricity . . . is equal to the square root of a length times a mass.' There is thus a triple ambiguity. In physics 'length' means, or should mean, the number l; this author uses the word to denote the ratio of

the two numbers l_1 and l_2; whereas his argument depends on taking the word to signify the ontological spatial attribute Length. Of course, as is evident from the experiments proving Coulomb's law, there exists something objective with which we associate the number q. Suppose that this consists of n electrons, and let the charge-measure of each be e. Then the law becomes $f = nn'e^2/\alpha r^2$. Here, in principle if not in fact, n and n' are numbers ascertainable by counting. And with each electron we associate the *number* e, which, if we like, we can call the electronic charge or 'quantity of electricity.' Whatever objective context we give to the equation, we cannot escape the conclusion that its symbols are measure-numbers. There is no possibility of thrusting the entities themselves—electrons or qualitative magnitudes—into our algebra; the attempt to do so is meaningless.

Yet it is this extraordinary idea which is behind all the juggling with dimensional metaphysics. We read in a recent number of *Nature*: [3]

> It has always seemed to me that there was no justification for regarding the three magnitudes, mass, length and time, as necessarily fundamental; and a system in which quantities expressed in those dimensions have fractional indices is unsatisfactory. . . . The introduction of a new dimension [Q, electricity] has automatically wiped out all fractional indices. . . . If M be regarded as a function of Q, the former would disappear entirely from the table; and everything in mechanics as well as in electricity and magnetism could be put in terms of Q, L and T.

We have already dealt with this horror of fractional indices, this preference for squares as against square roots. So let us concentrate on the last sentence. Taking $[\alpha] = L^2/T^2$, we have $Q = L^{\frac{1}{2}}M^{\frac{1}{2}}$. Apparently this is not nice, so we write it: $M = Q^2/L$, which of course we can use to eliminate M from any logometric formula. The writer can hardly have meant anything so elementary. Like most dimensionalists, he was under the impression that he was penetrating into the *arcana* of nature, whereas in fact he was dealing with the very simple algebra of measure-ratios. So, without any protest, it is actually proposed that even in mechanics, where neither q_1 nor q_2 exists, we should

[3] Prof. W. Cramp, *Nature*, 130 (1932) 368. *Cf.* Glazebrook, v. 247: 'It is of course true that if, as Prof. Cramp has pointed out, we treat a quantity of electricity as fundamental along with space, mass and time, the dimensional equations are simplified; and that further if we may treat all mass as electrical, the simplification is much more marked.'

still write the mass-ratio as Q^2/L! But an opponent appeared with this answer [4]:

> His argument is based on the assumption that Q shall be a function of M. Such an assumption would be a bombshell in modern physics. M, in common with L and T, is a quantity which varies with the velocity of the observer; Q does not so vary.

Sometimes one is tempted to wish for a few bombshells in modern physics. But in the present case we are dealing with very elementary and ancient principles. The more bombshells the dimensionalists hurl at one another's pronouncements, the sooner we may expect a return to sanity and reality. Let us try a little explosive common sense on the following controversy.

> If magnetic forces are due to electric currents, then the two standard equations $f = jj'dsds'/Ar^2$ and $f = mm'/\mu r^2$ must be co-dimensional; and the simplest solution is $m = jL$ and $A = \mu$ dimensionally. No objection either physical or mathematical has yet been offered to this solution, which eliminates from electrical science that great bugbear, the dual system of dimensions; and no sacrifice is required in adopting it.—Sir James B. Henderson, *Nature*, 139 (1937) 473.

Prof. G. W. O. Howe, who thinks $A = 1/\mu$, is horrified at Henderson's proposal. 'I can only express my surprise,' he says, 'that such a suggestion is put forward seriously.' When two learned dimensionalists differ so fundamentally, perhaps the modest suggestion that A is *neither* μ *nor* $1/\mu$ may find a hearing. Using f to denote any force-component and φ to denote a trigonometrical function, we have from (4.4, 5b)

$$f = \varphi/c^2\alpha \,.\, jj'dsds'r^{-2}.$$

Thus $A = c^2\alpha/\varphi$, where $\alpha = 1$ if we measure current in elst and $c^2\alpha = 1$ if we measure in elm. There is nothing further that can be usefully said about the coefficient. But if we wish loftily to prescind from the only measure-systems really employed, we can, in accordance with (4.4), put $A = a^2/\beta\varphi$. Taking $a = 1$ and *confusing* β with μ, we may put $A = 1/\mu\varphi$. So Prof. Howe wins a barren victory! Both antagonists, by the way, accept the formula $f = mm'/\mu r^2$ which we have rejected. As for the dual systems of dimensions being a 'great bugbear,' our view is that

[4] Prof. F. R. Denton, *Nature*, 130 (1932) 892. Lodge (i. 403) proposed $Q = L^2$. Auerbach (*Die Methoden der theor. Physik*, 1925, p. 12) proposed $Q = M$. $Q = L$ has also been proposed.—R. Weller, *Z. f. math. und naturw. Unterricht*, 64 (1933) 71.

all measure-ratios are arbitrary, infinite in number instead of being dual! Henderson's 'dimensional' equation $m = jL$, translated into our notation, is $[m] = JL$. Using (14.11, 12) we find that this means: $[a/\beta] = 1$, or $a/\beta = a'/\beta'$.

Starting from the elst-mag system ($\alpha = \beta = 1$, $a = c$), this implies that in our new system $a' = c\beta'$. But who wants this new system of measurement, even though there be no 'mathematical' objection to it? This little controversy is due entirely to a misunderstanding of the elementary symbols of physics.

A similar criticism applies to the statement that electric or magnetic intensity is a velocity:

> We treat it [electric intensity] as the velocity of an unknown but really existent motion. Hertz however defines it as the force which would act on unit charge. . . . In the ideal measure-system: $[E] = lt^{-1}$.—Boltzmann, ii. 13, 15.
>
> I can correlate most things in one scheme if I am allowed that magnetic force is velocity of the ether.—Larmor (1893), cited in Lodge, *My Philosophy*, 1933, p. 177.
>
> Magnetic induction is a kind of velocity in the aether.—Livens, ii. 241.

This language ceases to have any meaning as soon as we establish that physical symbols are measure-numbers and that dimensions are the ratios of these measures. At one stroke we have got rid of these futile speculations. There is one instance, however, the equation of resistance to velocity, which requires further examination.

Let us apply our formula for a change of units to charge q whose measure-ratio is

$$Q = M^{\frac{1}{2}}L^{\frac{3}{2}}[\alpha^{\frac{1}{2}}]/T.$$

Let us use the suffixes 1 and 2 for the elst ($\alpha_1 = 1$) and elm ($\alpha_2 = 1/c^2$) c.g.s. system, keeping q for the measure in the system: α, l cm., m gram, t sec. Then we have

$$q_1 = q_2 c = q m^{\frac{1}{2}} l^{\frac{3}{2}}/t\alpha^{\frac{1}{2}},$$

all the symbols being numbers and α quite arbitrary. This formula follows at once from the expression for Q. But we should have obtained the same expression for Q if we started, say, with $q^2 = mlv^2/\alpha$ instead of $f = qq'/\alpha r^2$. This shows us how useless it is to describe the elst unit as cm.$^{3/2}$ gm.$^{\frac{1}{2}}$/sec., even if we adopt the previously made suggestion for finding a meaning in this conglomeration of symbols. It only tells us in what ratio

the measure varies when we alter the units; and very different quantities may have the same measure-ratio. The logometric formula gives us no information whatever concerning the operational context in which the measure receives its meaning and definition. It is quite true that the elst unit is simply the number *one*. Yet when we say that a charge is q electrostatic units, we are saying much more than that the charge is the number q. We are tacitly referring to a mode of measuring which is assumed to have been previously agreed upon and to be briefly described by the adjective 'electrostatic.' The reference thus implied is made explicit by citing the formula $f = qq'/r^2$, taken in its metrical significance. The law of force is therefore an essential ingredient of the real meaning of such a phrase as 'q elst units,' it supplies the necessary operational or mensurational context.

Turn now to resistance (r), whose measure-ratio is $R = T/L[\alpha]$. In addition to the previous notation let us use the suffix 3 to denote the 'practical' system[5]: $l_3 = 10^9$ cm., $t_3 = 1$ sec., $\alpha = l_3^2/t_3^2c^2 = 10^{18}/c^2$. We have

$$r_1 = r_2/c^2 = 10^9 r_3/c^2 = rt\alpha/l. \tag{15.3}$$

We are now in a position to examine some peculiar statements concerning resistance:

> The [elm] dimensions of [R] are . . . [L/T] or those of a simple velocity. This velocity, as was pointed out by Weber, is an absolute velocity in nature quite independent of the fundamental units in which it is expressed. . . . The [elst] dimensions of [R] are [T/L] or the reciprocal of a velocity. Electric resistance in electrostatic units is measured by the reciprocal of an absolute velocity.—Maxwell-Jenkin, p. 76.
>
> In the electrostatic system . . . the resistance of the conductor is of the dimensions [$L^{-1}T$]. . . . In the electromagnetic system . . . a resistance is a quantity of the dimensions of a velocity and may therefore be expressed as a velocity.—Maxwell, i. 402, 466.
>
> The ohm [is] the resistance measured by 1,000,000,000 centimetres per second. I am afraid that conveys a strange idea, but it is perfectly true as to the absolutely definite meaning of resistance.—Kelvin, *Popular Lectures*, 1 (1891) 97.
>
> Every resistance is capable of being expressed as a velocity.—S. P. Thompson, p. 348.
>
> We find that the resistance of a conductor to an electrical current may be expressed as a velocity. Yet it would be absurd to attach any concrete relation between electrical resistance and mechanical velocity. . . . In this case analogy between physics and mathematics

[5] See further on, p. 808.

entirely fails, and no idea even hypothetical has been attached to the result.—L. T. More, *The Limitations of Science*, 1915, p. 154.

In the electromagnetic system a resistance is a quantity homogeneous with a velocity; and may therefore be expressed as velocity. —Maxwell, v. 177.

Absolute resistance has the dimensions of space/time.—Rowland, *Am. J. Sci.* 15 (1878) 290.

In 1869 Maxwell (in Maxwell-Jenkin, p. 76) gave: $c = 28 \cdot 798$ ohms $= 288 \times 10^6$ metres per second. In 1878 Rowland (*l.c.*, p. 439) stated: The final result of the experiment is 1 ohm $= \cdot 9911$ earth quad./sec.

To any unbiassed reader these statements, in spite of the authority of their writers, are patently absurd; and any system of alleged reasoning which leads to them stands self-condemned. The statements are not even consistent—for how could the same quantity be a velocity or the reciprocal of a velocity *ad libitum*? This conclusion should help to dispose finally of the dimensional mysticism on which it is based. The measure-ratio of resistance is $R = T/L[\alpha]$. If we keep to elst or to elm measure, $R = 1$; for the simple reason that we do *not* change our α or our units of length and time. If we wish to change from elst to elm, $L = T = 1$, $\alpha_1 = 1$ and $\alpha_2 = c^{-2}$; hence $r_1 = r_2/c_1^2$. Changing from elm to practical we have $r_2 = 10^9 r_3$. Nowhere do we meet this mysterious velocity. The metrical specification of the number r is given by the equation $r = V/j$. The measure-ratio is $T/L[\alpha]$ and might apply to a measure $r = 1/v\alpha$ if such a quantity occurred. How then did these statements arise? The explanation is very simple. Put $[\alpha] = 1$ and we get $R = T/L$; put $[\alpha] = L^2/T^2$ and we get $R = L/T$. Call the first supposition 'electrostatic' and the second 'electromagnetic.' Then interpret these logometric formulae as giving the qualitative essence. It follows that the essence of elst resistance is a slowness and that of elm resistance is a velocity. But it must now be evident that these premisses and conclusion are devoid of meaning.

2. Inductivity and Permeability.

The contemporary interminable discussions on 'dimensions' are based on the laws $f = qq'/\varkappa_0 r^2$ and $f = mm'/\mu_0 r^2$. Already in Chapter II we showed that the latter equation is incompatible with the existence of permanent magnets. We have also clearly demonstrated that the constants α and β should also occur in the denominators in addition to $\varkappa_0$ and μ_0, which do not replace them.

Against these alleged laws there are two other decisive arguments. In the first place, no one who professes to hold the electron theory seriously believes that vacuum possesses inductivity and permeability, for these quantities are defined as statistical properties of aggregates of electrons. It was far otherwise in Maxwell's time.

> Maxwell in fact chose to finally expound the theory by ascribing to the aether of free space a dielectric constant and a magnetic constant of the same types as had been found to express the properties of material media.—Larmor, iv. 620.
>
> In 1873 Maxwell effectively made the assumption that *empty* space and insulators contained elastically bound electric charges, capable of being displaced from their equilibrium position to an extent proportional to the strength of any applied electric field. When the field is applied, the charges move to their displaced positions and in so doing create a momentary displacement current.—Harnwell and Livingood, *Experimental Atomic Physics*, 1933, p. 4.

In spite of the lip-service given to displacement-currents, no one nowadays genuinely believes that empty space is full of elastically bound charges, still less that it is magnetised by being filled with Amperian micro-circuits. But the phraseology survives, especially among technicians, long after it has been denuded of meaning.[6] What is *really* meant is not $\varkappa_0$ and μ_0 but α and β. Perry wrote in 1891 :

> I would suggest . . . that the magnetic permeability of air shall no longer be assumed to be unity, but, in the practical system of measurement, be tabulated like the permeability of any other substance—the permeability of air will be $4\pi \times 10^{-9}$; and that in future no substance shall have unit permeability.—*Electrician*, 31 July 1891, p. 355 ; cited by G. A. Campbell, p. 74.

But clearly what he is really advocating is taking the arbitrary constant $\beta = 4\pi \times 10^{-9}$, which later was combined with $\alpha = 10^9/4\pi c^2$. Similarly Heaviside took $\alpha = \beta = 4\pi$, and in 1901 Giorgi proposed to put $\alpha = 10^7/4\pi c^2$, $\beta = 4\pi \times 10^{-7}$. These are arbitrary metrical decisions of a purely arithmetical nature, to be accepted or rejected merely on grounds of convenience ; they are entirely devoid of physical significance. In fact we have seen that if we do assume $\varkappa_0$ for vacuum, and incidentally reject the

[6] And of course it continues to be inflicted on the unfortunate students. *Cf.* q. 26 in Bridgman, iii. 109 : 'What are the dimensions of the magnetic permeability of empty space in the electrostatic [*i.e.* the useless 'max '] system of units ? What is its numerical value ? '

electron theory, we must take $\varkappa_0 \to \infty$. There are definite physical implications in assuming $\varkappa_0$ and μ_0 to be other than unity. It is not open to anybody to propose any values he pleases; such numerical arbitrariness applies only to the coefficients α and β.

In the following quotation from Giorgi we have changed $h = 1/\varkappa_0$ into $1/\alpha$ and $k = 1/\mu_0$ into $1/\beta$, in accordance with the arguments we have adduced.

> The new phase of science led us to regard the coefficients in the two fundamental formulae
>
> $$f = mm'/\beta r^2, \quad f = qq'/\alpha r^2$$
>
> as physical magnitudes corresponding to specific properties of free space or space-aether. . . . Maxwell showed that from the mathematical standpoint we can think that this energy is localised in the dielectric medium, and that for many reasons of a physical order this second conception is more natural than the first. These reasons constitute a good presumption but not an absolute proof; the definite proof came when electromagnetic waves became known. . . . There are two physical magnitudes [α and β], two specific properties of space; . . . space behaves like an extremely stiff spring. . . . Now in practical metrology no one would consent to take the velocity of light as unity. . . . Then why equate to unity one or other of the two physical coefficients α and β? And why one rather than the other? . . . The electrostatic system in putting $\alpha = 1$ hides the fact that empty space has very little susceptibility for being charged with energy in the electrostatic form; on the other hand, it gives to the other coefficient $1/\beta$ an extraordinarily large value, much larger than could be justified. With the electromagnetic system, the opposite happens. Both systems are incapable of helping us to appreciate the value of physical magnitudes and constants in a suitable manner. In the classical teaching the velocity of light, instead of being presented naturally as a function of the two fundamental properties of the medium in which the propagation takes place, is presented as a ratio between the electrostatic and the electromagnetic measurement of the same quantity—which is a veritable enigma for students.—Giorgi, ii. 465.

The reply to this can be tabulated as follows:

(1) The constants are not measures of specific properties of free space; nor does the existence of electromagnetic waves throw any light on their value which is quite arbitrary.

(2) The assignment of values to α and β is a question of convenience devoid of any theoretical implication.

(3) Giorgi ignores the elst-mag system ($\alpha = \beta = 1$) altogether,

though it is to be found in every text-book. Does he seriously think that there is some *theoretical* objection to putting $\alpha = 1$?

(4) If the simple equation $q_2/q_1 = c$ is 'a veritable enigma for students,' the only assignable reason is that their teachers are muddleheaded.

(5) Is it fair in discussing very elementary questions of mensuration to seek to impose Maxwell's reactionary and antiquated ideas upon us as if they were sacrosanct dogmas ? Is a man to be forbidden to use an ammeter because he declines to regard empty space as like a stiff spring ?

In the next place, $\varkappa$ and μ by their very definition are *necessarily* tautometric. So long as 'dimensions' could be regarded as mystic symbols conveying insight into the 'nature' of things, many extraordinary speculations could be made with impunity. But once we realise that we are dealing with prosaic measure-ratios, we have to revert to common sense. It was Sir Arthur Rücker who in 1889 decided to restore the unjustly 'suppressed dimensions' of $\varkappa$ and μ. He advocated (p. 108), 'the open admission in the symbols employed that the dimensions of some of the quantities are unknown.' To him is attributable most of the subsequent pseudo-metaphysics. And yet by substituting α and β for his $\varkappa$ and μ and by properly understanding 'dimensions,' his formulae became identical with ours and are susceptible of the most commonplace interpretation. Let us catalogue a few typical statements in order to give them their proper meaning :

> The dimensions of $\varkappa$ and μ are not definitely known, except that their product is $1/v^2$.—Kennelly, *Science*, 73 (1931) 535.
>
> We know nothing concerning the dimensions of the individual quantities μ and $\varkappa$, but only of their product.—A. Porter, *Method of Dimensions*, 1933, p. 69.
>
> The dimensions of μ are not known and most probably are unknowable.—Hague, p. 37.
>
> We cannot say what are the dimensions of μ_0 and $\varkappa_0$ in terms of mass, length and time.—Glazebrook, iv. 597.
>
> As to the separate values of $[\varkappa]$ and $[\mu]$, they are not known ; we take them qualitatively as non-existent and quantitatively as equal to unity.—C. Runge, *La mesure* (*Enc. Sc. Math.* tome v, vol. i, fasc. 1, 1916, p. 27).
>
> Separately μ and $\varkappa$ have indeterminate dimensions.—Loeb, p. 71.
>
> The recent work of committees both national and international upon the fundamental units and definitions has shown that a considerable further step in advance, by removing difficulties, would

result from the discovery of the dimensions of μ_0 and κ_0 . . . . Some writers have recommended that the dimensions of one of these should be chosen arbitrarily.—Henderson, ii. 105.

If in the future Nature divulges some secret whereby the limits of our knowledge are extended so that we are able to express μ in terms of L, M and T, the dimensional expressions can then be readily reduced to these three fundamentals. But in the meantime let us be honest with ourselves and not pretend to knowledge that we do not possess.—Howe, ii. 48.

We have here a perfect example of the ludicrous effects of juggling with undefined 'dimensions' and of the pathetic faith in some future 'discovery.' Apparently the writers claim to be dealing with the alleged equation (14.13), without quite knowing what it is supposed to mean. In reality they are dealing with (14.12), in which they arbitrarily put $[a] = 1$. It is quite true that the measure-ratios of α and β are 'unknown,' in the same simple sense that what I shall have for dinner to-morrow is unknown, i.e. until I decide. I can use any convenient α_1 and β_1 I like in combination with c.g.s. units. And if I decide to change my units, I can choose any α_2 and β_2 I like in the new system, so that $[\alpha] = \alpha_1/\alpha_2$ and $[\beta] = \beta_1/\beta_2$ are quite arbitrary, provided $[\alpha\beta] = T^2/L^2$. After the parturition of the mountains, there emerges this ridiculous mouse. The writers of text-books and the members of international congresses have been wasting their time on a pseudo-problem, whose futility is now apparent after we have got rid of mystic 'dimensions.'

We have already shown that equation (14.14)

$$1/[\kappa^{\frac{1}{2}}\mu^{\frac{1}{2}}] = L/T = U$$

is capable of a perfectly legitimate meaning, provided we interpret the symbols as denoting the ratios of the measures of corresponding quantities in two similar systems. This interpretation has of course no connection whatever with changes in units. It is seen that we are thus able to validate an equation which, as dimensionalists deduce and interpret it, is entirely erroneous. However, we must reject the assertion [7] that 'μ and κ are constants of unknown dimensions but such that $a^2/\mu\kappa = c^2$.' We must also reject all attempts to determine the separate 'dimensions' of μ and κ. For example, G. F. FitzGerald [8] in 1889 suggested (p. 253) $[\kappa] = [\mu] = 1/U$. 'There seems a naturalness

[7] SUN Commission in 1931 : ICP Report, p. 11.

[8] Followed more recently by Kennelly, *Science*, 73 (1931) 535.

in this result,' he says, 'that justifies the assumption that these inductive capacities are really of the nature of a slowness.' It is quite impossible to inject any meaning whatever into such a statement. All we can hope for is that the next generation of students of physics will not waste time speculating on the nature of a slowness! Not to be outdone, Sir Oliver Lodge (i. 403) enunciated in the same year among other 'modern views on electricity' this proposition: $[\kappa] = LT^2/M$, $[\mu] = M/L^3$. And another writer[9] advocated $[\kappa] = T^2/M$, $[\mu] = M/L^2$. 'These results,' he concludes, 'appear to throw some light on the nature of electric and magnetic phenomena.' He was mistaken; and unfortunately they throw no light on a much more curious phenomenon: the facility with which serious physicists can shuffle symbols without ever asking themselves what it is all about.

There seems to be still a difference of opinion between the theorists[10] and the technicians concerning the undefined 'dimensions' of μ. Becker (p. viii) speaks as follows in the preface to the eighth edition of Abraham's text-book:

> It does not seem possible at present to set up a system of units which will satisfy the electrical engineer and the physicist alike. With regard to Maxwell's theory, the difference between the physicist and the electrician is not a matter of notation merely, but of principle. The technical view adheres much more strictly than current physics does to the original form of the Faraday-Maxwell theory. The engineer looks upon the vectors **E** and **D**, even in a vacuum, as magnitudes of quite different kinds, related to one another more or less like tension and extension in the theory of elasticity. From this point of view it must of course seem a very questionable procedure, in an exposition of fundamental principles, to put the factor of proportionality κ, in the equation $\mathbf{D} = \kappa\mathbf{E}$, equal to 1 for empty space, thus artificially attributing to **D** and **E** the same dimensions. On the other hand, . . . the numerical identity of **E** and **D** for empty space in the Gaussian system of units is not for the physicist the result of an arbitrary definition, but the expression of the fact that **E** and **D** are actually the same thing. The introduction by the

[9] P. Joubin, JP 5 (1896) 398 f.

[10] It is pathetic to find a great physicist like Sommerfeld telling us (ii. 816) that the equation $\mathbf{F}/e = \mathbf{E} + c^{-1}V\mathbf{vB}$ 'would be a dimensional monstrosity with **H** instead of **B**'—even in the case when $\mathbf{H} = \mathbf{B}$. 'Naturally,' he says, 'the factor $\underset{0}{\mu} =$ permeability in vacuum must not be omitted,' not even when it is unity. Also (p. 815) 'even in vacuum we must not put $D = E$,' even when κ is one, we must not say it. 'Dimensions' in this case have become a mere fetich.

engineer of a dielectric constant and permeability not equal to 1 in a vacuum seems to the physicist to be merely an artifice, by means of which formulae are reduced to a shape which is convenient for practical calculations.

A writer in the *Reports on Progress in Physics*, 1934 (L. Hartshorn, p. 368) tells us :

> There has been much discussion of the question whether magnetic permeability is to be regarded as a quantity having dimensions or a mere number. . . . Among engineers there is a strong desire to regard the relation between magnetic force and magnetic induction as analogous to that between mechanical force and displacement and between electric force and displacement. Magnetic force and induction are therefore regarded as quantities of a different character, and a convention which assigns to them different dimensions is an aid to thought and is preferred.

Among the resolutions adopted by the International Electrotechnical Commission at Oslo in 1930 was one to the effect 'that the formula $\mathbf{B} = \mu_0\mathbf{H}$ represents the modern concepts of the physical relations for magnetic conditions in vacuo, it being understood that in this expression μ_0 possesses physical dimensions.'[11] A questionnaire sent round by the International Union of Pure and Applied Physics in 1932 asked : 'Should μ (the permeability) be treated as a quantity having dimensions in length, mass and time, or as a pure number ?' The British reply was that 'permeability should be regarded as a quantity having dimensions,' whereas the view of the Dutch Committee was 'that **B** and **H** are quantities of the same kind.'[12] An informal Conference was held in Paris in July 1932, consisting of members of the Union of Physics and of the Electrical Congress ; at this nine voted that **B** and **H** were 'quantities of a different nature,' three voted that they were the same in kind, the remainder abstained. What an amazing and humiliating position for physicists, contending about undefined terms, squabbling about the meaning of the simplest symbols, and counting heads ![13]

In view of our previous discussion, we can now make a few brief incisive comments on this painful exhibition. The symbols

[11] *ICP Report*, p. 16.

[12] E. Griffiths, *Nature*, 130 (1930) 987 f.

[13] 'In these days when the youngest among us are enthusiastic about ψ-functions and probability smears, it is not a little disturbing to find that an International Congress, held in Paris in July 1932, decided *by a majority vote* that "B and H are quantities of two different kinds."'—A. Ferguson, *School Science Review*, 18 (1937) 347.

of physics stand for pure numbers, they denote measures; they throw no light whatever on the nature of things. The man who executes the measures, the instrument-maker or the physicist in the laboratory, of course estimates the ratios of Lengths and Durations. The rest of us work with algebra, the symbols being placed in the context or background of ordinary experience. None of our measures *has* 'dimensions,' but if we change our units, we can find the ratio of two measures.

This measure-ratio constitutes the only intelligible meaning which can be assigned to the term 'dimensions.' The measure-ratio of permeability is necessarily unity; for by its very definition it is independent of our measures of length, time and mass. It does not follow that B and H are of the same nature or kind, for such terms are inapplicable to numbers. We therefore conclude that B, μ, H are pure numbers; that when we change our units (and the magnetic constant β), B and H are changed in the same ratio but μ remains unchanged. These conclusions are not altered one iota if technicians, rejecting the view of magnetism initiated by Ampère and adopted in the electron theory, regard vacuum as magnetic or invent some other picture of the magnetisation of a medium. The classical theory of polarisation still holds; so does the fundamental view that physics is concerned with basic and derived measures.

One would hesitate to oppose electrotechnologists in their own specialist domain. It is quite a different matter when they make pronouncements on the fundamentals of quantitative science and when they make ordinances concerning 'physical dimensions' which they have omitted to discuss or define. Hence we cannot accept the following dogmatic statement:

> There are still many people who do not agree to attribute different dimensions to the magnetic field and magnetic induction. But this distinction is now imposed definitely on engineers, who have, I believe, taken the excellent decision to replace the absolute permeabilities of magnetic substances by permeabilities relative to that of a vacuum. Hence we can attribute physical dimensions to this latter, we can without serious inconvenience choose for the μ_0 of empty space a power of 10 in non-absolute systems and even a factor 4π in systems of rational units.—A. Blondel, preface to Sudria, p. 5.

The latter part of the quotation directs our attention to another source of confusion. It seems clear that instead of the classical

definition $B = H + 4\pi I/\beta$, many recent writers take $B' = \beta H + 4\pi I = \beta B$, so that in vacuum $B' = \beta H$. Next, β is called μ_0', the 'permeability' of vacuum—an utterly inappropriate name for an arbitrary mensurational constant. Then $B'/H = \beta\mu$ is called μ', the 'absolute' permeability; the 'relative' permeability $\mu'/\mu_0' = \mu$ being the ordinary classical permeability. Of course we can 'without serious inconvenience' take β equal to a power of 10 or to 4π, though in fact $\beta = 1$ in all practical magnetic measurements. But it does cause serious inconvenience and misunderstanding to regard β as the permeability of vacuum; especially as no one nowadays seriously considers that vacuum is a magnetically polarised void.

The prevalent view may be summed up in Giorgi's objections to the equation $\mathbf{B} = \mathbf{H} + 4\pi\mathbf{I}/\beta$.

> There is a double irrationality: the assimilation of the physically heterogeneous quantities **B** and **H** from the standpoint of dimensions, and the apparition of 4π when there is no question of spherical bodies.—Giorgi, ii. 466.

To which we may retort that there is a double error in this objection. (1) The equation, being perfectly general, cannot possibly lack the algebraic property of homogeneity. In reality Giorgi accepts the equation as perfectly valid, but he prefers to define and employ a new quantity $\mathbf{B}' = \beta\mathbf{B}$. This would mean rewriting Chapter II and a good deal of electromagnetics; no reason is assigned for the proposed innovation. (2) He must, like every other writer, admit that the 4π *does* occur. But for some reason the factor irritates him. What he proposes to do about it is not very clear. Of course we might put $\beta = 4\pi$ and thus obtain $\mathbf{B} = \mathbf{H} + \mathbf{I}$. But then we should have $\mathbf{B}' = 4\pi(\mathbf{H}+\mathbf{I})$.

The Electrotechnical Congress decided that the mag unit of H should be called an *oersted* and the mag unit of B a *gauss*. This is a step which had for years been strongly advocated by American electricians; and their advocacy was based on the interpretation of physical symbols as something more than numbers and on the acceptance of dimensions as qualitative compendia. There were strong attacks on the idea that 'permeability is a mere numerical ratio and not a physical quantity'—as if it could not be both! We were told that 'B characterises the magnetised state of the medium and H is the agency tending to *produce* a magnetised state.'[14] But if we refer back to Chapter II

[14] Dellinger, p. 609 f.

we shall find that H and B (there called F and G) are simply measure numbers employed in connection with a system of singlets and doublets. There may be *in rerum natura* an agency producing a state; but this does not enter into our measure-numbers H and $H + 4\pi I/\beta$, which are calculated by combining experimentally ascertained ratios. In advocating a duplication of names, electrotechnologists frankly admit that they are not thinking at all of their own special and practical problems.

> If the dimensions of μ are zero so that μ is a simple numerical coefficient, it ought to follow that in the ordinary formula ($B = \mu H$) connecting magnetic force with density of magnetic flux, the unit chosen for B should have the same nature as that chosen for H, and the same name should be applicable to the two units; if one is expressed in gausses, so should the other. But if on the contrary μ has physical dimensions and is not a simple numerical coefficient, then B and H cannot strictly be expressed in the same unit.—Kennelly, i. 926.
>
> After considerable discussion in Copenhagen and Stockholm [in 1930], the committee [of the IEC] decided unanimously that for electrotechnical purposes the convention should be established that in free space the quantities flux-density B and magnetising force H should be taken as physically different; so that their ratio, the space permeability μ_0, was a physical quantity with dimensions and not a mere numeric.—Kennelly, v. 237.

In other words, there is no special authority or prestige attaching to the decision; in making it the electrotechnicians went outside their own domain and sought to impose on all physicists the peculiar views (1) that 'dimensions' have some intelligible (but unexplained) scientific sense apart from measure-ratios, (2) that the symbols of physics are not mere measure-numbers but have different 'natures.' As we have given cogent arguments against these views, we decline to accept the unwarranted decision which, it is alleged, 'is now imposed definitely on engineers.' Incidentally we deny that the 'convention' serves any 'electrotechnical purpose' whatever.[15]

[15] Says Sir James B. Henderson rather threateningly: 'I would remind Prof. Howe that one International Committee has already recommended that μ is to be considered a dimensional entity, and also that the final decision is to be made this year.'—*Nature*, 139 (1937) 473. 'I am in entire agreement with the recommendation of the Committee and with Rücker,' replies Prof. Howe. The present writer, on the other hand, is in entire disagreement, and he declines to submit even to an International Committee if it chooses to talk nonsense.

3. The Fourth Unit.

In this and the preceding chapter, indeed throughout this book, we have maintained that

> the phenomena by which electricity is known to us are of a mechanical kind, and therefore they must be measured by mechanical units or standards, . . . *all* electric phenomena may be measured in terms of time, mass and space only.—Maxwell and Jenkin, p. 60.

The point seems really self-evident to anyone who regards the fundamental formula $f = qq'/\alpha r^2$ without pedantic prepossession. It is true that α is an arbitrary number; but so is γ in the formula force $= \gamma$ mass $\times$ acceleration; we always put $\gamma = 1$ and we often take $\alpha = 1$. The employment of different values of α is purely a matter of metrical convenience, without any objective significance in what is measured. The constant has no connection with any peculiarities of electricity. We have already explained why the measures known as length, time and mass are termed basic. It is obvious that electric charge is not basic, for it is a number occurring in an equation containing these basic measures.

And yet, as the following quotations amply testify, this view is now supposed to be quite out of date. The unanimity of the chorus is impressive; even more so is the public recantation, made with due signs of repentance, which newly converted physicists have felt impelled to make. The worship of dimensions appears to excite some of the emotions more appropriate to a religious creed.

> Quantity of electricity must be treated as a fourth fundamental quantity, provisionally at least, for we are allowed to hope that one day we shall be able to determine its dimensions in [L], [M], [T].—H. Abraham, JP 1 (1892) 520.
>
> Maxwell attempted to express the measures of the various quantities occurring in terms of the three fundamental variables of mechanics—length, mass and time; and found that without further assumptions this was impossible. The fundamental electrical quantities are four in number.—Sir R. T. Glazebrook, iv. 597.
>
> In mechanics there are only three fundamental units—those of length, mass and time—and these three would probably serve for a system of electrical measurements if the identity of electrical and mechanical phenomena could be established.—Sir F. E. Smith, i. 211.
>
> The necessity for one additional unit arises from the fact that the

identity of mechanical and electrical phenomena has not been established.—Dellinger, p. 600.

In our present state of knowledge it is impossible to express the physical dimensions of electric and magnetic quantities in terms of mechanical units alone.—Karapetoff, p. 725.

It was generally believed, in the days when the basis of Physics was purely mechanical in the old-fashioned sense, that all physical units could be derived from the units of length, mass and time; and many electrical units for example are still described as c.g.s. units. . . . In all equations containing electric or magnetic quantities, we shall find two of them present in addition to those quantities which are derivable from length, mass and time. We are therefore forced to introduce an electric (or magnetic) fundamental unit.—W. Wilson, p. 38 f.

The founders of the c.g.s. system, which dates from 1873 and from which the systems of electrical units are derived, had doubtless thought they were setting up a system embracing the whole of physics. It was the epoch when it was universally admitted—and I myself shared the illusion during most of my life—that all the phenomena of physics were in the last analysis reducible to mechanics. Since then it has been vainly tried to explain electromagnetic phenomena by mechanics alone; but the conception of the electron has had to be admitted, and this means the employment, for electromagnetic phenomena, of a fourth fundamental magnitude in addition to those of mechanics.—Brylinski, *Revue gén. de l'électricité*, 8 Nov. 1930, p. 722.

The orthodox number three, which is at the basis of the so-called absolute measure-system, could appear obligatory so long as it could be hoped to reduce electricity to mechanics. This time is past. We do wrong to the electromagnetic magnitudes if we force them into the Procrustean bed of the three units. . . . After I had propagated this system [of three units] in my lectures and writings for 30 years, I have now, in my latest lectures on electrodynamics, changed over to the general system of four units. We shall in what follows employ electric charge as the fourth unit.—Sommerfeld, ii. 814.

The inspiration borrowed from mechanics, still dominant in 1870, has been laid aside; we have given up the idea of explaining all physical phenomena by mechanics. A beginning was made with the frank admission that in the facts of electricity there is something which cannot be reduced to $[L]$, $[M]$, $[T]$. We are now well aware that dimensional formulae are not connected with the intimate nature of things; they are the result of arbitrary conventions. But these conventions, in order to be useful, must be inspired by reasons of clarity, of opportuneness and of simplicity. Now none of these reasons can justify the setting up of such complicated formulae as $[L^{3/2}M^{1/2}T^{-2}]$ with fractional indices, nor the representation of resistance by velocity, nor that of capacity or inductance by length. Our physical knowledge seems to correspond to reality

much more surely and sincerely if we recognise the necessity of a fourth fundamental of electric nature. I say 'fourth fundamental' so that the phrase can include both a fundamental magnitude and an arbitrary fundamental unit.—Giorgi, ii. 464.

The first thing to get into our heads is that all this talk has not the remotest relation to any such idea as is expressed in the electromagnetic theory of mass; still less is it connected with any modern views of atomic structure. There is nothing modern whatever about it; we are dealing with exceedingly elementary physics; we have to do with simple equations known to Maxwell and known even to Gauss and Poisson. What has happened is not any revolutionary reconstruction of these simple formulae, not anything at all in *physics*, but something in *physicists*. And that is a distressingly accentuated preoccupation with the pseudo-mystical idea of 'dimensions.'

Now the science of physics is, especially in its elementary metrological aspects, essentially pragmatic and operational. The proper way to deal with suspiciously metaphysical problems, which physicists are so prone to foist into the science, is to ask: What practical effect has any possible answer in *physics*, in the structural elements and working formulae of the science itself? So here let us inquire into what, if any, consequence, internal to physics, is drawn from the assumption of this alleged fourth fundamental unit. The first thing we observe is that not a single existing formula is altered by one iota. None of the neo-dimensionalists dreams of denying that $f = qq'/\varkappa r^2$. Apparently all they want is to play the game of dimension-shuffling in a way different from the way it is played by the palaeo-dimensionalists of the Maxwell era. Inasmuch as, according to our previous arguments, the whole business has as much to do with physics as has chess or checkers, this particular dispute is also irrelevant.

Take, for instance, one of Sommerfeld's deductions from his new creed. 'No one will deny' the following, he tells us (ii. 815). From $f = qE$ he deduces $E =$ dyne/charge, from $4\pi q = \int D_n dS$ he deduces $D =$ charge/cm.^{2} From which he concludes that $\varkappa$ is 'a qualified number of the dimension charge^2/erg. cm.' He does not alter a single working equation of physics. He merely invents some new combinations of words and symbols which have no relevance to physics and which, taken literally, are either of the form $1^2/1 \times 1$ or are meaningless. But, not content with

this destructive criticism, we have been at pains to discover whether, within the range of physics, there happen to be simple formulae of which these hieroglyphics may be considered a parody. We had no difficulty in answering this question affirmatively—but only on the understanding that the undefined 'dimensions' of these writers were replaced by the numbers we have called measure-ratios. If we thus completely re-interpret the dimensionalists' alleged relations, we obtain quantitative formulae which are both simple and useful. Let us find the measure-ratios corresponding to Sommerfeld's manipulations. The equation $f = qE$ is a definition; it gives $[E] = F/Q$. His next equation is not general, it must in accordance with (2.8a) be expressed as $4\pi q/\alpha = \int D_n dS$; this gives $[D] = Q/[\alpha]L^2$. His conclusion is now expressed as

$$[D/E] = Q^2/[\alpha]FL^2.$$

Undoubtedly this is the measure-ratio of $\varkappa$, due to a change of units. But Sommerfeld appears to have overlooked the elementary equation (2.2) : $E = -\nabla\varphi/\alpha$. This gives

$$[E] = Q/[\alpha]L^2 = [D].$$

Accordingly the measure-ratio of $\varkappa$ is unity, and $Q^2 = [\alpha]FL^2$, as is obvious from $f = qq'/\alpha r^2$. Hence when any alleged application of this theory of 'four units' is translated into the intelligible metrical formulae of physics, it is found to disappear. The discussion, in so far as it turns on 'dimensions,' is simply a waste of time.

We must now deal with an extremely practical application which, according to electrotechnologists, follows from the adoption of the four-units hypothesis. Reversing the former policy of practical standards, technicians are now desirous of adopting elm-mag units, or rather various decimal multiples thereof. But they have become convinced, chiefly by vague talk about the fundamental nature of electricity, that this is impossible without adopting 'a fourth fundamental unit.' It is not at all clear what, if any, is the scientific content of this conviction. But these practical electricians are unanimously of opinion that it means something; only they differ as to *which* fourth unit they should 'adopt.'

There was considerable difference of opinion among the delegates as to the fourth fundamental unit for the system. The ohm and the

coulomb each had been suggested. It was agreed that a fourth unit was needed, because it would be possible, starting with the three units metre, kilogram and second, to construct an indefinite number of possible associated electromagnetic series, differing from the existing practical series which all desired to maintain. It was finally agreed to defer action on the choice of a fourth fundamental unit.—Kennelly, v. 239, referring to discussions of the EMMU (Electric and Magnetic Magnitudes and Units) Committee of the IEC (International Electrotechnical Commission) at Scheveningen near Brussels in 1935.

The theory of physical dimensions was beginning to be better understood ; and the opinion was no longer held that everything in the physical world depended necessarily on three fundamental dimensions. Physicists recognised that entropy, temperature, loudness of a sound, light intensity, etc., brought into play some dimensions which were not dependent on $[L]$, $[M]$, $[T]$. Why ought not electric and magnetic magnitudes to be treated in the same way ? . . . Accordingly, the principle of having a fourth fundamental dimension entering into the electric and magnetic magnitudes gradually became universally recognised. . . . This may be any one of the electric or magnetic magnitudes, for instance, the quantity of electricity $[Q]$. . . . Any one of the electrotechnical units may be taken as fundamental, and all others become derived units.—Giorgi, pp. 5 f., 9 (pamphlet issued by the IEC in 1934).

One would have to be very optimistic to maintain that the theory of so-called dimensions is ‘ beginning to be better understood.’ [16] The fact that the principle of a fourth dimension is now ‘ universally recognised ’ is certainly not one of the symptoms of understanding. What exactly did the technicians think they were doing when they decided ‘ to defer action ’ ? *What* action ? The reference to the ohm and the coulomb suggests that they were thinking of reverting to practical standards specified by a voltameter or a mercury column. Such a decision would have no theoretical reactions. We cannot therefore agree with the following statement :

Electrical phenomena cannot be reduced to mechanical processes or interpreted mechanically. This knowledge has become generally accepted. Nevertheless even to-day men fail to see a necessary consequence of this fact, namely, the general introduction of the

[16] Giorgi's own views are permeated with the false ideas we have refuted. After pointing out the alleged dimensional contradictions between the ‘ electrostatic ’ and the ‘ electromagnetic ’ systems—he instances the ‘ dimensions ’ of resistance as being LT^{-1} and $L^{-1}T$—he refers thus to the Gaussian [i.e. elm-mag] system (ii. 460) : ‘ All that was not sufficient to resolve the contradiction ; instead of two metrologies we had three.’

international electrical measure-system. It seems strange, but this system is still simply unknown in many physical circles. Even leading text-books confuse it with the 'practical' system which cannot serve for physical purposes.—Pohl and Roos, p. 3.

This is merely a grandiloquent way of saying that there is some profound metaphysical difference between (1) the relation connecting current-measure and the mass electrolytically deposited, and (2) the relation between, say, current-measure and the torque on a permanent magnet. The ordinary physicist will fail to see any profound difference. For reasons of historical development and theoretical formulation we generally start from (2) and proceed to (1). But there would be no objection in principle to taking our unit of charge as the charge per chemical equivalent of metallic salt in solution, i.e. the *faraday* equal to 9650 elms or 9650 c elsts; or we might take any fraction of it, and we could accordingly define unit current. All that really matters is that we have to deal with *both* phenomena; the setting up of an electrolytic standard of measure helps our convenience and the standardisation of instruments, it has no theoretical or metaphysical influence whatever.

But there is a contradiction. We cannot adopt *both* the elm unit (or one-tenth of it) *and* the voltametric unit, unless of course the latter is regarded as an approximate standardised realisation of the former. Moreover, as we pointed out in Chapter II, the General Conference of Weights and Measures has adopted 'absolute units,' in the sense of discarding 'standards'; and this will become legal in many countries by the beginning of 1940. To avoid further confusion, it is to be hoped that the *IEC* will 'defer' indefinitely any action contrary to this decision.

It is possible, however, to give a more charitable interpretation to these discussions of the technicians. Perhaps they were merely debating what value they should adopt for one or more of the constants a, α, β? These three constants entered electromagnetics as follows:

$$f = qq'/\alpha r^2;$$

$$f = mm'/\beta r^2;$$

$$C = \beta j/a, \text{ or } (4.3): H = 2\pi j/ar.$$

For the elm-mag system $a^2/\alpha\beta = c^2$; and if we change the units of length and time, we must take $[a^2/\alpha\beta] = L^2/T^2$. We can

therefore say that, in addition to a choice of the units of length, mass and time, we have in electromagnetics the further *arbitrary* choice of the values of the two independent constants α and β. If this is what was meant at these international gatherings—let us hope it was—then, instead of speaking of the choice of a fourth fundamental unit, the discussion should have centred on the choice of two arbitrary values (α and β). And if this was not what was meant, it is time that someone should state what—within the range of physics—it was all about.

Having disposed of the question of units, we can now turn to the other interpretation of measure-ratios in order to elucidate what is obscurely at the back of the minds of those physicists who are clamouring for a fourth unit in electricity. What they are really thinking of appears to be the patent macroscopic fact that charge can be varied independently of lengths, masses and times. What they forget of course is that this will alter the mechanical forces. In this, charge differs from temperature; for—speaking macroscopically and apart from laws giving the variation of quantities with the temperature—the temperature can be varied without altering a mechanical system. Now we have done justice to these facts by declaring that charge and temperature are *characteristics* for appropriate similar systems. In this sense we can add Q and Θ to the three measure-ratios L, M, T. In this sense, and in this sense only, charge might be termed a fourth basic quantity. We have already shown the meaning of this by simple applications. Let us now show that any result which *seems* to follow from the four-units hypothesis is really a misinterpretation of some result applicable to similar systems, i.e. the so-called 'dimensions' are being unwittingly used for measure-ratios in this second sense. An example from Sommerfeld (ii. 815) will suffice. He gives the following 'formula' for specific conductivity:

$$\sigma = \frac{1}{\text{cm. res.}} = \frac{\text{charge}^2}{\text{dyne}} \cdot \frac{\text{cm.}^{-2}}{\text{sec.}}$$

'Our dimensional formula,' he says, 'points directly to the explanation of σ in the electron theory: $\sigma = e^2nl/mv$,' where n is the number of electrons (e) per unit volume and l is their mean free path. 'The dimensional formula written in four units expresses much more than that written in three units; for example in Lorentz's system $\sigma = \text{sec.}^{-1}$.'

Let us find the necessary measure-ratios for two similar systems :

Formula $f = qq'/\alpha r^2$ gives $Q^2 = FL^2$.
Formula $Vq =$ work gives $Q[V] = FL$.
Formula $V = j\rho$ gives $QR/T = [V]$.

Whence

$$R = FLT/Q^2.$$

This may be contrasted with the logometric formula (14.11) : $R = T/L[\alpha]$, where of course the letters have entirely different meanings [measure-ratios for a change of units and α].

The resistance of a wire (length l and cross-section A) is specific res. $\times\ l/A$ or $l/(A \times$ sp. cond.), i.e. $\rho = l/A\sigma$. Hence

$$\begin{aligned}[\sigma] &= 1/RL \\ &= Q^2/FL^2T \\ &= \frac{Q^2}{F} \cdot \frac{L^{-2}}{T}.\end{aligned}$$

And this is the physical analogue of Sommerfeld's metaphysical 'formula.' In order to find the analogue of the curious so-called Lorentzian formula '$\sigma = \text{sec.}^{-1}$,' we must of course revert to the first interpretation of measure-ratios. From

$$R = T/L[\alpha] \text{ and } [\sigma] = 1/RL$$

we obtain

$$[\sigma] = [\alpha]/T,$$

in which we may, if we so decide, take $[\alpha] = 1$.

We have therefore ousted all the meta-physical mysteries. We have not only vindicated common sense, we have also provided all the proposed formulae with simple meanings accessible to any schoolboy, entirely too elementary to be the subject of discussions and votes at learned international congresses.

4. Practical Measures.

We are now in a position to give further consideration to some points connected with the practical units already discussed in Chapter II, section 8. The International Conference of 1908, proceeding to define the ohm and the ampere, declared :

> The Conference agrees that as heretofore the magnitudes of the fundamental electrical units shall be determined on the electro-

magnetic system of measurement with reference to the centimetre as the unit of length, the gram as the unit of mass and the second as the unit of time.—*Reports of the Committee of Electrical Standards appointed by the British Association*, 1913, p. 753.

On the other hand the EMMU (Electric and Magnetic Magnitudes and Units) Committee of the IEC (International Electrotechnical Commission) decided, at the meeting held in Paris in October 1933,

> to invite the national committees to give their opinion on the extension of the series of practical units at present used in electrotechnics by its incorporation in a coherent system, having as fundamental units of length, mass and time the metre, kilogram and second and as fourth unit either that of resistance expressed as the precise multiple 10^9 of c.g.s. electromagnetic unit or the corresponding value of the space permeability of a vacuum.—Kennelly, v. 238.

And the IEC—fifteen countries being represented by the delegates present—at its plenary meeting held in June 1935 at Scheveningen near Brussels, unanimously adopted the Giorgi system of metre-kilogram-second (m.k.s.) units. The question of 'rationalisation' (i.e. the insertion of 4π in α) was deferred for future consideration.[17]

We must now investigate this decision, its alleged reasons and consequences. Let us begin by examining the effect of the general system of measure-ratios (due to change of units and constants)

$$L = 10^x,\ M = 10^y,\ T = 10^0 = 1,\ [\alpha] = 10^z,\ [\beta] = 10^u, \qquad (15.4)$$

applied to the electrical and magnetic equations. The following table exhibits the results for a transformation from the elm-mag system :

[17] In November 1935 Glazebrook (cited in *Engineering*, November 6, 1936, p. 498) proposed 'that the "fourth unit" on the m.k.s. system be 10^{-7} henry per metre, the value assigned on that system to the permeability of space.' That is, in ordinary language, he merely advocated taking $\beta = 10^{-7}$. According to Prof. Marchant—*Nature* 136 (1935) 110—at the I.E.C. meeting in Scheveningen (Brussels): (1) 'The adoption of an m.k.s. system with four fundamental units' was unanimously accepted, i.e. presumably m.k.s. units with $\alpha = 1$ and $\beta = ?$ 'It was agreed that the m.k.s. system should have units which were consistent with the c.g.s. system'—which seems like a contradiction in terms. (2) It was decided to hold consultations on 'the choice of the fourth fundamental unit'—quantity or resistance being mentioned. 'The committees to be consulted should be asked to give a value for the fourth unit which was consistent with the value of permeability of free space being equal to unity.' Not a difficult task, for the result is secured by definition !

Quantity.	*Measure-ratio.*	*Exponent of* 10.	*Pra-system.*	*Max-well.*	*Giorgi.*	*Vario-rum.*
	$[a]$	$(2x+z+u)/2$		0	0	0
energy	W	$2x+y$	7	7	7	7
charge or current	$Q=J$	$(3x+y+z)/2$	-1	-1	-1	-1
e.m.f.	$[V]$	$(x+y-z)/2$	8	8	8	8
resistance	$R=[V]/J$	$-(x+z)$	9	9	9	9
capacity	$C=[q/V]=1/R$	$x+z$	-9	-9	-9	-9
inductance	$[L]$	x	9	9	2*	0*
pole-strength	$[m]$	$(3x+y+u)/2$		8	8	8
magnetic intensity	$[H]$	$(-x+y-u)/2$	-1	-10*	-3*	-1
magnetic induction	$[B]$	$(-x+y-u)/2$	8	-10*	-3*	-1*
m.m.f.	$[F]=[H]L$	$(x+y-u)/2$	-1	-1	-1	-1
flux	$[N]=[B]L^2$	$(3x+y-u)/2$	8	8	1*	-1*

(15.5)

The first column specifies the quantity, the second gives the symbol for the measure-ratio. The third column gives the exponent of 10 for the measure-ratio in question. This is easily obtained. Equation (14.12) is

$$[a/\alpha^{\frac{1}{2}}\beta^{\frac{1}{2}}] = L/T.$$

Hence from (15.4)

$$[a] = L[\alpha^{\frac{1}{2}}][\beta^{\frac{1}{2}}] = 10^{(2x+z+u)/2}.$$

The other exponents are similarly obtained from (14.11). For instance

$$[H] = L^{-\frac{1}{2}}M^{\frac{1}{2}}[\beta^{-\frac{1}{2}}] = 10^{(-x+y-u)/2}.$$

The fourth column, giving the practical system, is taken from (2.47 and 55). For example $q/q' = 10^{-1}$, $r/r' = 10^9$, where the unprimed letters refer to the elm-mag system and the dashed letters denote the practical measures.

The fifth column specifies a transformation (applied to the elm-mag system) which seems to have been first suggested by Maxwell (ii. 268), who gives it as the choice of the units : $l = 10^9$ cm., $m = 10^{-11}$ gram, $t = 1$ sec. However the accurate specification is as follows :

$$x = 9, \quad y = -11, \quad z = -18, \quad u = 0. \qquad (15.6)$$

This means that

$$
\begin{aligned}
&10^{-18} = \alpha/\alpha_1 = 1/c^2\alpha_1, \text{ or } \alpha_1 = 10^{18}/c^2\,; \\
&\beta_1 = \beta = 1\,; \\
&a_1 = a = 1\,; \\
&a_1^2/\alpha_1\beta_1 = c^2 10^{-18}\,;
\end{aligned}
$$

where, as before, the unprimed letters refer to the elm-mag system, but the letters with the suffix 1 refer to this 'Maxwell' transformed system.

The sixth column is the transformation first proposed by Giorgi in 1901 and now adopted by the IEC. It is specified by

$$x = 2, \quad y = 3, \quad z = -11, \quad u = 7. \qquad (15.7)$$

Hence [18]

$$
\begin{aligned}
&10^{-11} = \alpha/\alpha_2 = 1/c^2\alpha_2, \text{ or } \alpha_2 = 10^{11}/c^2\,; \\
&10^{7} = \beta/\beta_2 = 1/\beta_2, \text{ or } \beta_2 = 10^{-7}\,; \\
&a_2 = a = 1\,; \\
&a_2^2/\alpha_2\beta_2 = c^2 10^{-4}\,;
\end{aligned}
$$

where the symbols with the suffix 2 refer to measures in the Giorgi system.

The seventh column contains the results of what we may term the Variorum transformation,[19] which has been suggested by various people—Bennett, Blondel, Dellinger, Karapetoff, Mie and others. This transformation is

$$x = 0, \quad y = 7, \quad z = -9, \quad u = 9. \qquad (15.8)$$

Hence

$$
\begin{aligned}
&10^{-9} = \alpha/\alpha_3 = 1/c^2\alpha_3, \text{ or } \alpha_3 = 10^{9}/c^2\,; \\
&10^{9} = \beta/\beta_3 = 1/\beta_3, \text{ or } \beta_3 = 10^{-9}\,; \\
&a_3 = a = 1\,; \\
&a_3^2/\alpha_3\beta_3 = c^2.
\end{aligned}
$$

Now the professed object of each of these three transformations is to produce a system of measures identical with the pra-system. But on comparing the last four columns of table (15.5) we discover that there are discrepancies; these are marked with an asterisk. Also, surprisingly, Maxwell's system emerges best, with only two discrepancies; whereas Giorgi's has four and the

[18] Kennelly (v. 242) gives α_2 (which he calls ϵ_0) as $10^7/c^2$. Giorgi (i. 14) takes ϵ_0 (*i.e.* α_2) as $(10^9/4\pi c^2)$ farad/metre, and $\mathring{\mu}$ (*i.e.* β) as $4\pi \times 10^{-7}$ henry/metre.

[19] *Cf.* E. Bennett and H. Crothers, *Introductory Electrodynamics for Engineers*, New York, 1926, pp. 12, 433, 639.

Variorum transformation has three discrepancies. Let us see the condition that the transformation gives the correct measure-ratios for

(1) W, i.e. $2x + y = 7$;
(2) Q (i.e. $3x + y + z = -2$) or V (i.e. $x + y - z = 16$) or R (i.e. $x + z = -9$);

and (3) keeps a unity i.e. $[a] = 1$ so that $2x + z + u = 0$. The answer is

$$y = 7 - 2x, \quad z = -9 - x, \quad u = 9 - x.$$

Maxwell's corresponds to $x = 9$, Giorgi's to $x = 2$, the Variorum to $x = 0$. But any value of x will satisfy these three conditions. We can choose x so that one other condition is satisfied; for example, $x = 0$ makes $[H] = 1/10$, $x = 9$ satisfies $[N] = 10^8$. If we abandon the proviso $[a] = 1$, we can choose a transformation which will make both $[H] = 1/10$ and $[N] = 10^8$; namely, $x = 9/2$, $y = -2$, $z = -27/2$, $u = -9/2$. But this makes $[L] = 10^{9/2}$ and $[B] = 1/10$. No transformation can simultaneously satisfy all the conditions. In particular the conditions concerning $[B]$ and $[H]$ cannot both be fulfilled, for these two numbers are necessarily equal. Presumably it is for electrotechnicians to say how much they want to 'pra' the oersted and the gauss; and apparently they desire different pra-ratios. The practical men are the best judges of what is convenient as regards units in different equations. But they are not entitled to say that their pra-system, as at present formulated, is derivable from the elm-mag consistent system by means of an L—M—T transformation combined with a change in α and β. For we have just shown that this is impossible; and our argument is valid even against Giorgi's transformation in spite of its recent authoritative adoption.

The matter is really obvious if we examine such a phrase as that used by Grimsehl-Tomaschek (pp. 125, 376): 'the practical unit of magnetic induction is 1 volt . sec./cm.^{2},' i.e. 10^8 gauss. If we consistently adopt the new units we should say that the pra-gauss is volt/(pra-cm.)^2—we may omit the 'sec' which is unity. In the case of Maxwell's units, this is 10^{-10} gauss, since his pra-cm. is 10^9 cm. In the case of the other two transformations, the case is complicated by the omission of factors. The general formula is (4.30)

$$V = -\frac{\beta}{a}\frac{dN}{dt}.$$

For Maxwell $\beta_1 = a_1 = 1$, $V_1 = V'$, $N_1 = 10^{-8}N = N'$, hence his transformation correctly gives $V' = -dN'/dt$. But $B/B_1 = [B] = [N]/L^2 = 10^8/10^{18} = 10^{-10}$, whereas $B/B' = 10^8$. In the Giorgi system we have $\beta_2 = 10^{-7}$, $a_2 = 1$, $V_2 = V'$, $N_2 = 10^{-1}N = 10^7 N'$. Hence Giorgi's equation is

$$V_2 = -10^{-7} dN_2/dt,$$

which is of course the same as $V' = -dN'/dt$.

For Giorgi $B/B_2 = [N]/L^2 = 10/10^4 = 10^{-3}$ so that $B_2 = 10^3 B = 10^{11} B'$.

Similarly equation (2.49)

$$V = -\frac{\beta}{a^2} L \frac{dj}{dt}$$

becomes

$$V_1 = -L_1 dj_1/dt$$

in Maxwell's system, and

$$V_2 = -10^{-7} L_2 dj_2/dt$$

in Giorgi's.

Again, consider the general equation (4.3)

$$H = 2\pi j/ar.$$

For Maxwell: $j_1 = j'$, $a_1 = 1$, $r/r_1 = 10^9$; also $H/H_1 = 10^{-10}$ and $H/H' = 10^{-1}$, so that $H_1 = 10^9 H'$. Hence the equation $H_1 = 2\pi j_1/a_1 r_1$ is identical with $H' = 2\pi j'/r$.

For Giorgi: $j_2 = j'$, $a_2 = 1$, $r/r_2 = 10^2$; also $H/H_2 = 10^{-3}$ and $H/H' = 10^{-1}$, so that $H_2 = 10^2 H'$. Hence the equation $H_2 = 2\pi j_2/a_2 r$ is identical with $H' = 2\pi j'/r$.

Now these transformations, and in particular Giorgi's, have obviously been proposed as being identical with, and hence superseding, the practical system. But if our schedule of discrepancies in (15.5) is correct, this claim is unfounded. Let us therefore glance at Giorgi's pamphlet to discover the origin of what we maintain to be a serious error. On p. 12 we read that 'magnetic flux . . . is that quantity the rate of decrease of which with respect to time equals the induced e.m.f.' In other words, the equation $V = -dN/dt$ is taken as valid in all systems of units with all values of β and a. Whereas, as we have already shown, the general equation is

$$V = -\beta/a \,.\, dN/dt.$$

The omitted factor β/a has the following values: 1 in the elm-mag system, $1/c$ in the elm-max system, $1/c$ in the elst-mag system, $1/c^2$ in the elst-max system, 10^{-7} in Giorgi's system. Thus Giorgi has omitted the important factor 10^{-7}. Ultimately his error is traceable to the prevalent version of the principle of equivalence: $C = j/a$ instead of $C = \beta j/a$. It is indeed obvious that the quantitative equivalence of magnets and currents must involve the constant β of the magnetic force-law $f = mm'/\beta r^2$. We have therefore justified the retention of this constant in order to deal with the case in which it is not made unity; and we have thereby been enabled to point out a hitherto unnoticed, and really fatal, defect in the Giorgi system which has been prematurely adopted by the IEC.

Accordingly we must reject Giorgi's contention:

> Magnetic flux . . . is that quantity the rate of decrease of which with respect to time equals the induced e.m.f. Therefore it is a product of a voltage into a time, with dimensions $[VT]$. . . . The unit accordingly is the *volt-second*. . . . The name proposed for it by the International Committee is *weber* (p.12).

The measure-ratio of flux is really

$$[N] = [V]T[a/\beta]$$
$$= [V/\beta] = \text{say, } 10^b,$$

since in the present case $T = [a] = 1$. From table (15.5) we see that $b = (x + y - z - 2u)/2$. And this is equal to the exponent $(3x + y - u)/2$ given in the table for $[N]$; since in this case $[a] = 1$, or $2x + z + u = 0$. Hence the elm-mag equation $V = -dN/dt$ becomes[20] in Giorgi's system $V_2 = -10^{-7}dN_2/dt$. But, as we have seen, $V_2 = V'$; hence N_2 is *not* equal to N'. That is, Giorgi's transformation fails to give the practical measure of magnetic flux (i.e. based on the weber as unit). The same holds for magnetic induction which Giorgi calls 'the magnetic flux per unit area.' For $[B] = [N]/L^2$, so that $B_2 = 10^3B$. It is therefore untrue to say with Giorgi (p. 12) that 'the resulting unit is the weber per square metre, . . . it corresponds to 10^4

[20] The situation becomes worse if, with Giorgi, we 'rationalise' the units. For then we have $V_2 = -4\pi \times 10^{-7}dN_2/dt$, and $N_2 = N/4\pi \times 10 = (10^7/4\pi)N'$. Note that 'flux' is merely the integral $\int B_\pi dS$. There is 'no experimental law of any reasonable precision in which flux is a constant. And if we have not measured flux, how can we determine its rate of decrease?'—N. Campbell, viii. 716.

gausses of the c.g.s. system.' On the contrary, the Giorgi unit of induction is 10^{-3} gauss.

The 'dimension' which Giorgi assigns to what engineers call by the peculiar term 'magnetomotive force' corresponds to our measure-ratio

$$[F] = [H]L = J/[a] = J.$$

The magnetic intensity (or force) is then defined as 'the m.m.f. per unit length of path,' i.e. $[H] = [F]/L$. To a physicist this seems a roundabout way of getting this measure-ratio. For Giorgi's transformation we obtain $H/H_2 = [H] = 10^{-3}$, so that (as we already pointed out) $H_2 = 10^2 H'$. Hence the Giorgi unit of magnetic intensity is one-hundredth of the practical unit (oersted).

We therefore conclude that all these recent attempts at representing the practical system as derivable from elm-mag measures by a consistent transformation, are a failure. We have not really advanced a single step beyond the account given in Chapter II. The rock on which every transformation, even Maxwell's, must split is the assignment of different numerical values (10^{-1} and 10^8) to the two quantities $[H]$ and $[B]$, which, in any consistent measure-set, must necessarily be equal. The pra-oersted and the pra-gauss (or weber) remain hybrid.

We must now deal with a possible answer to our criticism. As already pointed out, a very objectionable new nomenclature has been creeping in, to the complete confusion of any logical or consistent treatment of electromagnetics. Recent writers have invented a new 'magnetic induction' $b = \beta B$. The quantity $\beta\mu$ is called 'the absolute permeability' and μ is called 'the relative permeability' or the ratio of $\beta\mu$ to μ. It is only in this Pickwickian sense that the following statement is true:

> In practical measurements we are usually concerned with the relative permeability, which is the ratio of the permeability of the material to the permeability of a vacuum.—Vigoureux and Webb, p. 276.

In other words, we measure μ which is the ratio of $\beta\mu$ to β! Once more we meet the untenable identification of β with the permeability of vacuum. In the same sense presumably $b = \beta B$ is the 'absolute' induction and B is the 'relative' induction. Since in Giorgian units $B_2 = 10^3 B$, the 'absolute' induction is $b_2 = 10^{-4} B$; and the unit of 'absolute' induction is 10^4 gauss.

Apparently this is what Giorgi meant by his statement quoted above. So while we measure 'relative' permeability we measure 'absolute' induction. The factor β must now be hidden away and we have to rewrite the whole theory. Thus

$$V = -\frac{\beta}{a}\frac{d}{dt}\int B_n dS = -\frac{\beta}{a}\frac{dN}{dt}$$

becomes

$$V = -\frac{1}{a}\frac{d}{dt}\int b_n dS = -\frac{1}{a}\frac{dn}{dt},$$

where $n = \beta N$ is the 'absolute' flux. Similarly the equation

$$V = -\frac{\beta}{a^2}L\frac{dj}{dt}$$

becomes

$$V = -\frac{1}{a^2}l\frac{dj}{dt},$$

where $l = \beta L$ is the 'absolute' inductance. That is, in order to accommodate these Giorgian units we have to start an extensive process of concealing the arbitrarily chosen factor β; it has to be tucked away by a series of new definitions.

Against this procedure we have given strong arguments which we shall now briefly recapitulate.

(1) The constant β is not really got rid of, the force-law still remains $f = mm'/\beta r^2$. But it is assumed that there exists an infinite homogeneous soft magnetic medium of permeability $\mathring{\mu}$, so that the apparent law of force is $f = mm'/\beta' r$, where $\beta' = \beta\mathring{\mu}$.

(2) Utterly diverse proposals have been made concerning the most useful value to give to this constant. These suggestions really refer to the arbitrary constant β', and not to $\mathring{\mu}$. The latter cannot possibly be arbitrary, it is completely determined by the magnetic constitution of the hypothetical medium.

(3) It is extremely doubtful whether anyone nowadays seriously believes in such a medium filled with magnetic doublets or Amperian whirls. It would seem rather that the attribution of permeability to empty space is due merely to careless terminology. The authors [21] just cited for their belief in 'the permeability of a vacuum' tell us on the previous page that 'in accordance with present-day views on the constitution of the atom, this change of flux is to be regarded as due to the superposition' of the field of

[21] Vigoureux and Webb, p. 275.

' the molecular currents ' on the field ' applied by the magnetising winding.' That is, magnetism is taken as *exclusively* due to molecular currents.

(4) This is confirmed when it is observed that those who employ the new phraseology have no intention of denying the existence of permanent magnets.

(5) The equation

$$\mathbf{B} = \mathbf{H} + 4\pi\mathbf{I}/\beta,$$

together with Poisson's analysis leading thereto, are, in one form or another, universally admitted. The presence of a soft-magnetic medium is not provided for by simply multiplying every term in the equation by β, as is asserted by exponents of Giorgian units. For, as we have seen in Chapter II, the apparent $\mathbf{H}' = \mathbf{H}$ and

$$\mathbf{B}' = \mu'\mathbf{H}' = \mu/\mu_0 \,.\, \mathbf{H} = \mathbf{B}/\mu_0.$$

So the equation becomes

$$\mathbf{B}' = \mathbf{H}'/\mu_0 + 4\pi\mathbf{I}/\beta'.$$

(6) Prescinding from this difficulty, let us apply the principle of homogeneity to the equation. We have for the measure-ratios

$$[B] = [H] = [I/\beta].$$

This proves that the correct formulation of the law of equivalence is $C = \beta j/a$, as we have given it. For

$$J[\beta/a] = [C] = [I]L = [\beta H]L.$$

Hence

$$[H] = J/[a]L.$$

And this also follows at once from the *admitted* equation

$$H = 2\pi j/ar.$$

(7) It follows that

$$V = -\beta/a \,.\, dN/dt$$

is homogeneous (' dimensionally ') and that the equation $V = -1/a \,.\, dN/dt$ is correct only when $\beta = 1$.

(8) Even if technicians were justified in asking such a heavy price—the complete confusion of the proofs and exposition of electromagnetic theory—in order to patch up their unnecessary Giorgian units, they fail in the end. For H_2 is still a hundred times the practical H', instead of being equal to it.

Moreover, we cannot accept the following resolutions adopted

by the IEC at Oslo in 1930 as an adequate attempt to adjust the theory :

> The magnetic flux density B is a vector which represents in magnitude and direction the state of total polarisation due to a magnetic field. . . . The formula $B = \mathring{\mu} H$ represents the modern concepts of the physical relations for magnetic conditions in vacuo, it being understood that in this expression $\mathring{\mu}$ possesses physical dimensions. In the case of magnetic substances the above formula becomes $B = \mu H$, in which μ has the same dimensions as $\mathring{\mu}$. It follows that the specific or relative permeability of a magnetic substance is a number equal to $\mu/\mathring{\mu}$.—ICP Report, p. 16.

The vague definition of B is quite useless ; if it means anything, it implies acceptance of Poisson's analysis. The 'modern concepts' referred to are in reality quite out of date. We have, we trust, got rid of 'physical dimensions.' The Poisson equation $B = \mu H$ is given ; and then it is tacitly assumed, without the smallest attempt at proof or theoretical reconciliation, that in all experiments we really measure $\mu' = \mu/\mathring{\mu}$. And all this is apparently done—empty space is given a permeability of 10^{-7}—so that Giorgi's transformation may be forced to yield the practical measures. Is it worth it ?

The inconsistency in the units of magnetic intensity and magnetic induction, which is apparently desired by the practical men, has unfortunately led recent German writers to increase the already widespread confusion of theory. Mie writes as follows :

> In vacuum $B = \mathring{\mu} H$. It is one of the most important tasks of physical measurement to find accurately the universal constant $\mathring{\mu}$. . . . In purely magnetic measurements the so-called c.g.s. units are yet almost always used. There would be no objection to this if it were not usually conjoined with a very harmful confusion of ideas, which we wish to point out clearly in the hope that the necessary accuracy and order will gradually prevail in this great and important department of physics. . . . The units of the c.g.s. system are so chosen that the unit of energy is the erg and the permeability of vacuum is 4π. . . . Throughout the literature the greatest confusion prevails ; . . . and $\mathring{\mu}$, because it is taken as equal to 4π, is wrongly reckoned to be a pure dimensionless number. . . . The usual replacement of $\varkappa_0 = 1/\mathring{\mu} c^2$ by $\varkappa_0 = 1/4\pi c^2$ is misleading.—Mie, pp. 154 f., 156, 158.

The writer's intentions are good, but his equipment in the fundamental principles of metrology is insufficient. We have

already shown that in a vacuum B is *necessarily* equal to H in *any* system of units; for the very simple physical reason that a vacuum does not contain any of those Amperian microscopic currents whose statistical effect is represented by the factor we call permeability. Hence if a writer finds that B is *not* equal to H in vacuum, the reason is merely that he is not using consistent units on both sides of the equation $B = xH$. There is nothing very reprehensible in this, provided we understand what we are doing. What is really intolerable is to call this factor $\overset{\circ}{\mu}$, as if it were the permeability of vacuum, and to dub it a universal constant of physical importance. First B is measured neither in gauss (mag system) nor in what should logically be the pra-mag system, but in the commonly used hybrid unit volt . sec./cm.^{2}, which is really 10^8 gauss, so that $B' = 10^{-8}B$, where B is in gauss. Next for H (in oersted or gauss) there is substituted a new quantity H' related to it by the relation $H = (4\pi/10)H'$. Naturally the identity $B = H$, where both quantities are measured in the same units, now becomes

$$B' = 4\pi \times 10^{-9}H'.$$

First by a mixture of measure-systems the factor 10^{-8} is introduced; next the factor $4\pi/10$ is interpolated just because some practical engineers like to talk of ampere-turns. Then both factors, of purely mensurational significance, are multiplied together; and we are told that the product is a universal constant which is is one of the most important tasks of physical measurement to find accurately! [22]

Similarly we can deal with the metamorphosis of the equation $D = \varkappa E$, which is the analogue of $B = \mu H$. According to equation (2.36), $E = 4\pi\sigma/\varkappa\alpha$. Therefore if, following these writers, we define a new quantity $D' = \sigma$, we have $D' = (\varkappa\alpha/4\pi)E$. In the elst system $\alpha = 1$, in the elm system, $\alpha = 1/c^2$. Let us now see how these ingredients are combined to produce a bewildering medley.

> The dielectric constant of vacuum is equal to the ratio of the two measures of one and the same electrical field in vacuum, when D is in one case measured in coulomb/cm.^{2} and E in another case as volt/cm.—Mie, p. 438.

[22] Tomaschek (p. 659) admits frankly that his μ is merely $4\pi \times 10^{-9}$. But Pohl and Roos (p. 10) assert that this identity has 'not the smallest physical or geometrical significance'!

First we take $D' = \sigma' = 10\sigma/c$ in coul. per cm.^{2}, where σ is in elst. Next for E in elst we substitute $E' = (c10^{-8})E$ in volt/cm. Then the simple elst equation $E = (4\pi/\varkappa)\sigma$ becomes

$$D' = (10^{-9}\varkappa/4\pi c^2)E.$$

When we are dealing with vacuum ($\varkappa = 1$), the factor becomes $8{\cdot}86 \times 10^{-14}$, which is then called the absolute dielectric constant of the aether! Thus by juggling with the definition of electrostatic induction and by manipulating the units of measurement, we transform a perfectly intelligible and straightforward proposition into a mysterious relationship supposed to give us 'the dielectric constant of vacuum' for which we have already taken $\varkappa = 1$. It is an instructive example of misplaced ingenuity and perverted terminology.

The confusion is by no means confined to writers of one nationality. Witness this quotation:

> Since B is measured in volt-seconds per square metre and H in ampere-turns per metre, the permeability μ has the dimensions volt-second per ampere-metre, in other words of henry per metre. . . . In building up the system of practical electric units, it had been planned to choose the ohm such as to make the permeability of vacuum exactly equal to $4\pi \times 10^{-7}$ henry per metre. . . . By international agreement its adjustment to that value is likely to be effected by the year 1940.—Vigoureux and Webb, p. 3.

Let us sincerely hope that by the time 1940 comes we shall have heard the last of these 'dimensions,' and that the absurdity of talking of the 'permeability of vacuum'—henrys per empty metre—will be generally recognised.

5. Magnetic Units.

In view of the increasing outcry [23] against such non-existent monstrosities as 'magnetic poles,' we must now supplement the remarks we made in Chapter II. We propose to investigate Giorgi's statement (i. 11) that 'magnetic units are derived from electric ones.'

We start with the simple proposition that we all admit the existence of magnets. 'We begin,' says Maxwell (ii. § 606), 'by admitting the existence of permanent magnets.' The relevant

[23] Typical articles are F. W. Warburton's 'The Magnetic Pole a Useless Concept,' *Amer. Physics Teacher*, 2 (1934) 1–6; and D. L. Webster's 'Facing Reality in the Teaching of Magnetism,' *ibid.* pp. 7–10.

experiments, dating from Coulomb and Gauss, are described in every text-book. And every student of practical physics has had to use such an instrument as a tangent galvanometer. So now the rather obvious question arises : *Where* do we get the required measures for dealing with magnets ? Is it from 'electricity' ?

Adjusting our notation, we found in (2.2) the 'field' or intensity due to a magnetic doublet of moment M,

$$\mathbf{H} = -1/\beta \,.\, \nabla_0(\mathbf{M}\nabla p).$$

The formula for a change of units is given by

$$[H] = [M]/[\beta]L^3 = [m]/[\beta]L^2,$$

where m is the 'pole-strength.' And the change-ratio for force is

$$F = [mH] = [m^2]/[\beta]L^2,$$

corresponding to the force-law $f = mm'/\beta r^2$. Are we thereby assuming the 'physical existence' of poles ? We certainly are not. But we are as entitled to the interim use of poles in our mathematical analysis as we are to the employment of waves of potential or probability.[24] It is curious that this squeamishness about poles is increasing just at the time when, at the instigation of quantum theorists, physicists are becoming quite reckless about interim analytic hypotheses. It is even more curious that at the present time physicists, led by relativists who in other directions are so sceptical, seem to be insisting more than ever on the reality of 'fields' and even on the quasi-substantiality of 'magnetic lines of force.' Einstein himself tells us that

> we are constrained to imagine, after the manner of Faraday, that the magnet calls into being something physically real in the space around it, that something being what we call a 'magnetic field.'—*Relativity*, 1920, p. 63.

If 'we' are 'constrained' to 'imagine' this, we have no right to boggle at magnetic poles. For the 'field' at any point of 'space' is simply the vector **H**, which is defined by the equation $m\mathbf{H}$ = mechanical force on a pole m at the point. There may possibly be some alternative definition ; but it would involve enormous circumlocution and nobody has ever attempted it.

Of course, by the time **H** reaches a measurable formula, it must be associated with verifiable quantities ; for instance, by

[24] Moreover, according to (1.24), *any* vector-field whatever may be regarded as due to a distribution of Newtonian singlets and doublets.

means of formula (2.12a) which gives $-(\mathbf{MH})$ as the potential energy of another doublet **M** in the 'field' **H**.

(i) Let us see how the subject is dealt with in the recent text-book of Grimsehl-Tomaschek. The turning-moment on a doublet M in a field F is obviously $FM \sin\theta$; which is evident from the elementary consideration that we have two forces $\mp mF$ acting at a perpendicular distance $l \sin\theta$, and $M = ml$. This is accepted for an 'electric dipole.' We are then told (p. 121) that 'experiment has shown that a corresponding relation exists for bar magnets,' i.e. the couple is $HM \sin\theta$—the authors writing B for H. This, of course, is merely an ingenious disguise for admitting magnetic dipoles. Moreover it is admitted (p. 126) that MH and M/H are measured by 'magnetometers,' i.e. by permanent magnets. It is then rather a surprise to read this declaration (p. 128):

> Historical reasons account for the nature of the attempt to treat magnetic phenomena quantitatively by means of relationships analogous to Newton's law of gravitation. . . . It was Faraday's power of unbiased independent thought that first produced the theory of field action—a theory far more in harmony with the observed phenomena.

This invocation of Faraday as a tutelary deity is presumably intended to gloss over the fact that the ordinary Newtonian formula for force or intensity has been used. A mere change of nomenclature—'field' instead of intensity—does not display independent thought or produce different quantitative laws.

Let us next examine how the authors of this text-book proceed to electrical phenomena. It is 'decided to define the unit of current-strength electrolytically' (p. 193)—which is contrary to the existing international agreement. That is, the current which, when flowing uniformly through a silver coulombmeter, deposits 1·118 milligrams of silver per second, is called one ampere (p. 155). Then in connection with a long solenoid, of n turns and length l, there is *defined* a new quantity $H' = nj/l$, where j is the current measured in amps (p. 177). The authors apply the name 'magnetic induction' to what we call H, and what we refer to as H' they call 'field-strength'; the terminology is peculiar, though it is becoming common in German text-books; but the argument is unaffected by idiosyncrasies in epithets. 'The definition of field-strength in ampere-turns per cm. is not

dependent upon the force exerted upon a unit pole.' Quite true; and in so far as it is true, it means that H' is a perfectly useless combination of n, j and l. But experiments on the couple exerted on a permanent magnet by the solenoid are now described. And (p. 178) 'the result of the above experiments—namely that the force effects within a solenoid are proportional to the number of ampere-turns per centimetre—indicates that—in space devoid of matter, to which the quantitative relationships so far obtained all relate—the magnetic induction is proportional to the field-strength.' In plain English, the new-fangled H' is proportional to the old-fashioned H, used previously in the same book. This was to be expected, for (j being in amps) the magnetic intensity inside the (long) solenoid is given by

$$H = 4\pi nj/10l = (4\pi/10)H'.$$

So after all this unnecessary interlude, we have returned to H—and to magnetic poles. And we have had to assume a law connecting currents *and* magnets, each independently investigated and measured.

We have been obliged to re-translate the results, for the equation just given is in the text-book expressed in the form

$$B = \mu_0 H, \text{ where } \mu_0 = (4\pi/10)10^{-8}.$$

After what we have said in the preceding section, the misleading terminology of this equation needs no further criticism. But it is worth while quoting the following remark:

> Before the relationships discussed in the foregoing paragraphs had been expressed in the clear form which Maxwell gave to Faraday's intuitive mental pictures, efforts were directed towards the description of electromagnetic phenomena in terms of laws formulated on the pattern of Newton's law of gravitation.—Grimsehl and Tomaschek, p. 190.

On which we may comment as follows: (1) The 'clear form' is anything but evident! (2) The 'mental pictures' are a pure irrelevancy and do not result in the alteration of a single formula. (3) Newton's pattern has not been superseded, it is merely overlaid.

(ii) We shall examine another attempt. According to Pohl and Roos (p. 5), in the international system, unlike all previous

systems of units, current and potential are 'fundamental magnitudes.' Hence they 'are measured electrically, i.e. by comparison with a unit current and unit potential-difference—this is the essential characteristic of the international system of measurement.' A current, according to Pohl (p. 12), is measured electrolytically, one amp liberating $1 \cdot 1180$ mgm. of silver in one second. Now this measurement is neither 'fundamental' nor 'electrical'; it is derived and mechanical; the current is taken to be proportional to the mass or weight deposited. In thus measuring a current, we do not directly compare it with a unit current; we weigh the silver it deposits in a given time (t sec.), and we compare the mass with $1 \cdot 1180 \times t$ mgm.—which *ex hypothesi* is correlated with a current whose measure is to be unity. That is, current-measure is $j = Cm$, where m is the mass of silver in milligrams, and C is taken to be $1/1 \cdot 1180$. There is nothing revolutionary about this; the principle is clear since the time of Faraday. But whereas he started with the magnetic method of measuring current and experimentally proved the electrolytic or mechanical (mass) method, we are now asked to start with the electrolytic—which is much less sensitive and accurate—and to deduce experimentally the magnetic measure. But however we proceed, we require both measures.

Once more 'the magnetic field in a long coil' has to be assumed. Pohl (p. 92) defines it as $H' = Anj/l$; and the innovation of taking $A = 1$, instead of the usual $4\pi/10$, is apparently regarded as having some significance. But why do we combine these measures in the form nj/l? How 'long' must the coil be? And how are we to 'define' the magnetic field for other forms of circuit as well as for the earth and for a bar-magnet? And next we find that after all a permanently magnetised needle has to be employed. So long as nj/l is kept constant, the coil exerts the same torque on the needle—a result known since the time of Ampère. But, we are told, the needle 'is not used for *measuring* fields with, but merely for establishing the *equality* of two fields.' In fact 'the angle of torsion . . . is a measure of the turning-moment exerted on the needle by the field.' But obviously we require more than that, we have to graduate the magnetometer, to correlate its deflection (θ) with the magnetic field H', i.e. must assume or verify the formula: torque $= CMH' \sin \theta$. Having got this far, we realise that we are assuming for magnets the same mathematical formulae which

Pohl previously admitted for the 'polarisation of a dielectric' (p. 59).

(iii) Curtis's book on *Electrical Measurements*, published in 1937, has as its sub-title: 'Precise Comparisons of Standards and Absolute Determinations of the Units.' In other words, the Principal Physicist at the Bureau of Standards has written a work on very refined practical metrology. It is unfortunately inevitable just now that any such book must start from the very confused theoretical formulations which are prevalent. It begins thus (p. 5):

> In the definitions which follow, this fiction of a magnetic pole is not used; but instead the electromagnetic force of attraction or repulsion of two conductors carrying currents is taken as the starting point.

On p. 6 the 'fundamental law' is taken to be formula (4.8), with μ inserted (without definition). We have already shown that, according to the electron theory which the author presumably accepts, the correct formula is (4.12d = 11.6a) and the *fundamental* law is Liénard's force-formula.

'The two basic magnetic concepts,' we are told (p. 16), 'are magnetic intensity and magnetic induction.' Whereas of course they are not new or basic 'concepts' at all. The 'fundamental law' for H is, as a particular case, formula (4.3): $H = 2\pi j/ar$, where the author puts K for $1/a$. And the 'fundamental law' (p. 18) for B is given in a form equivalent to (4.30): $V = -\beta/a \,.\, dN/dt$, where the author now puts K for β/a. These so-called fundamental laws are discharged at us like bullets from a rifle, with no indication as to their metrological context or theoretical interconnection. This habit of the staccato interjection of isolated postulates may be all right in the symbolic game which very pure mathematicians call geometry; but it is an unmitigated nuisance in the science of physics.

A few pages later (p. 20 f) we read:

> The poles of a solenoid are the two points, one at each end, which appear to be the source of the external field of magnetic induction. . . . The pole-strength of a long solenoid of cross-section dS, having n turns of wire per cm. through which a current J in c.g.s. units is flowing, is equal to $nJdS$ provided the solenoid is in a vacuum. . . . If the solenoid is immersed in a medium of permeability μ, then the flux and the pole-strength are μ times the value in a vacuum. . . .

> With some magnetic materials in the solenoid, the flux does not decrease to zero when the current becomes zero. These materials are then permanent magnets, and the pole-strength is defined as the magnetic flux divided by 4π.
>
> The unit magnetic pole was originally defined as that pole which would repel an equal pole placed at a distance of 1 cm. from it with a force of 1 dyne. This is equivalent to the preceding definition and gives a better mental picture. However, it is necessary to use the one given to develop a logical system of units when the magnetic units are based on the electrical units.

So after all we *do* come to permanent magnets—and even to those terrible 'poles.' But, in the alleged interests of an unspecified 'logic,' we have to approach them in a roundabout way. Unfortunately the whole method is exploded by the next remark (p. 22):

> The first step in the direction of connecting the electrical units with the mechanical units was taken by Gauss in 1832. Gauss devised a method, which is still in extensive use, for measuring the horizontal intensity of the earth's magnetic field in terms of length, mass and time.

But Gauss's experiments were made with permanent magnets and had no reference to electricity. So once again it becomes clear that practical metrologists must willy-nilly accept independent magnetic units and measurements. And there is no reason, historical or logical, why they should not do so.

(iv) An informal conference was held in Paris in 1932 between representatives of the International Union of Physics and of the National Committees of a number of countries. It was agreed [25] that the system of magnetic units may be based on one of the following:

(*a*) The force between two magnetic poles (Coulomb).
(*b*) The force between two current-elements (Ampère).
(*c*) The measurement of magnetic flux.

It is not very clear what is meant by this proposition, for on p. 11 of the ICP Report we find an appendix on 'alternative methods on which to base electromagnetic quantities'—no longer merely *magnetic* quantities. Under method (*a*) three equations

[25] ICP Report, p. 5. 'There was no decided majority in favour of any one of these.'—E. Griffiths, *Nature*, 130 (1932) 988.

are given. These we reproduce as in the Report and in our notation :

(1) $f = ee'/\varkappa_0 r^2$: $f = qq'/\alpha r^2$.

(2) $f = mm'/\underset{0}{\mu} r^2$: $f = mm'/\beta r^2$.

(3) $f = mi \sin \theta ds/Ar^2$: $\mathbf{H} = j/a . \int V \mathbf{dsr}_1/r^2$.

In other words we substitute α and β for $\varkappa_0$ and $\underset{0}{\mu}$, and we integrate the last equation over a complete circuit. The reasons for so doing have been already argued at length and need not be repeated.

The next statements cannot be accepted without correction :

> The forces being measured in free space, practically in air, whence $A^2/\underset{0}{\mu}\varkappa_0 = (\text{velocity})^2$. The velocity can be shown to be that of electromagnetic waves. Also A is constant for all media. Maxwell puts $A = 1$ and alternatively $\varkappa_0 = 1$ (electrostatic system) or $\underset{0}{\mu} = 1$ (electromagnetic system). In Gauss' system $A = c$, the velocity of wave-propagation in free space, while $\underset{0}{\mu}$ and $\varkappa_0$ are pure numbers having no dimensions. In a more general case $\underset{0}{\mu}$ and $\varkappa_0$ are constants of unknown dimensions but such that $A^2/\underset{0}{\mu}\varkappa_0 = c^2$. . . . The magnetic flux through any circuit is connected with the e.m.f. E set up in the circuit by changes in the value of φ by the equation $\varphi = -\int Edt$ [i.e. $V = -dN/dt$]. . . . We have generally that in free space $B_0 = \underset{0}{\mu} H$ and in a medium in which the permeability is $\underset{1}{\mu}$, $B_1 = \underset{1}{\mu} H$. Thus $B_1/B_0 = \underset{1}{\mu}/\underset{0}{\mu} = \mu$ the specific permeability.—ICP Report, p. 11 f.

According to the arguments developed in this book, the correct statements are as follows :

(1) For the c.g.s. systems in usage, $a^2/\alpha\beta = 1/c^2$, where c is the ratio of elst to elm measure of charge. Theory—the electron theory in either Liénard's or Ritz's formulation—confirmed by experiment, shows that c is the (numerical) velocity of light.

(2) In the elst system $\alpha = 1$, in the elm system $\alpha = 1/c^2$.

(3) In the mag system $\beta = 1$, in the max system $\beta = 1/c^2$. The latter system does not appear to be ever used.

(4) The only two combinations we need consider are :

	α	β	a
elst-mag	1	1	c
elm-mag	$1/c^2$	1	1

(5) The constants α and β are not only pure numbers but have arbitrary measure-ratios ('dimensions'). However we cannot also change a arbitrarily, for $[a^2/\alpha\beta] = L^2/T^2$. In point of fact —except in discussing Giorgi's proposal—we never do want to take these measure-ratios or to deal with L and T (and even Giorgi takes $T = 1$).

(6) The general formula for the e.m.f. of induction is

$$V = -\beta/a \,.\, dN/dt.$$

(7) The 'magnetic induction' in free space is $B = H$, and not $B = \beta H$.

The second method (*b*) professes to deduce the electric and magnetic units from Ampère's relation, which is given in the Report (p. 12) in a form equivalent to

$$\mathbf{dF} = jV\mathbf{dsH}, \text{ where } \mathbf{H} = j' \int V\mathbf{ds'r}_1/r^2.$$

But according to (4.1) and (4.5a), this should be in general

$$\mathbf{dF} = \beta j/a \,.\, V\mathbf{dsH}, \text{ where } \mathbf{H} = j'/a \,.\int V\mathbf{ds'r}_1/r^2,$$

where β and a are constants to be fixed by our units, and **H** is merely an auxiliary vector defined by the integral over the closed linear circuit which, in the experiments, is a metallic wire.

Restoring these factors and changing the notation, we can now deal with the next step in the argument.

> From the point of view developed here, it is possible to describe all magnetic phenomena as due to the action of atomic currents, without the introduction of any other notion than that of magnetic induction. At any given point of a magnetic body a distinction is drawn between the induction due to distant currents, which varies gradually from point to point, and the induction produced by local currents. . . . We are thus led to consider the mean total induction at a point, which is the sum of the mean induction due to all the local currents, and the induction due to the distant currents.—ICP Report, p. 12.

That is,

$$\mathbf{B} = \mathbf{H} + 4\pi\mathbf{I}/\beta.$$

The second vector is 'proportional to the mean of the vector $j\mathbf{dS}$, dS being the area of the circuit in which the local current j circulates' (p. 13*). Or, in accordance with (4.8a), **I** is the volume-mean of

$$\mathbf{dM} = \beta j/a \,.\, \mathbf{dS}.$$

The reluctance to say simply that each micro-circuit acts as a magnetic doublet, following on the initial resolve not to say that a macro-circuit is found to be equivalent to a magnetic shell, is rather hard to understand. For we are next told that we must assume 'the constant μ_0 introduced by the formula of the older theory $f = mm'/\mu_0 r^2$, which expresses Coulomb's law.' The constants assumed 'are connected by a relation which follows when the expressions for the force between two magnets in the classical and the new notation are equated'; anticipating this, we have used the constants β and a from the start. But if we have to assume or introduce 'the force between two magnets' and the couple on 'a magnet of magnetic moment **M**,' what becomes of the claim to deduce all magnetic and electric measures and units from 'the force between two elements of current' *alone*? Obviously, the claim has evaporated, and **H** from being an auxiliary integral has become the magnetic intensity of the 'classical' or 'older' theory.

Method (*c*), 'based on magnetic flux,' is defended and explained as follows:

> The object of this system is to provide a method of deriving a system of units that shall be free from theoretical abstractions like the unit pole, and that shall be capable of verification by the ordinary student in an ordinary laboratory. . . . Electrical quantity is regarded as fundamental and its unit is based on electrolytic effects (the coulomb). . . . The e.m.f. unit is derived from the heating effect of a current (the volt). . . . The unit of flux density [magnetic induction] is that flux which when removed from a turn embracing one sq. cm. sets up unit electromagnetic momentum (the volt second). The unit of field-intensity is one current-turn per cm. . . . The whole of the above can be experimentally demonstrated in the laboratory, and no appeal is made to permanent magnets or magnetic poles. The former are regarded as special solenoids, and the latter as places where in a magnetic circuit the character of the medium changes.—ICP Report, pp. 13–16.

It is true that magnetic pole (or rather magnetic doublet) is in one sense a 'theoretical abstraction.' But this is even more true of 'magnetic flux,' and especially of 'electromagnetic momentum.' Apparently the idea is to drop all the quantitative concatenation of the various phenomena, and to set students making isolated experiments on electrolysis, heating of wires, induction, etc. If so, it would clearly be a very retrograde step, entirely opposed

to the present international attitude towards the elm-mag system. The idea that magnetic experiments—which are also capable of verification by the ordinary student in the ordinary laboratory—are included, is surely a delusion. What is the unfortunate student to think when he is told that the earth's magnetic field—measured, say, with a magnetometer—is so many current-turns per centimetre? There seems to be no objection made to regarding a magnet as a special solenoid (which it is not); but for some reason it would be against this 'method' to regard a solenoid as a magnet, though this latter proposition is far more accurate and intelligible.

It is indeed difficult to see what exactly is the object of these last two methods. If their purpose is to secure a complete system of units without mentioning or considering magnets, they are obviously a failure. As *practical* devices, they may be ruled out; no one would dream of following them. It is only in *theory* that we can eliminate magnets. We find from Ampère's experiments that we can *imitate* a magnet by an electric circuit. So we adopt the theory that all magnets are ultimately due to the presence of microscopic electric currents. Let us put it quantitatively:

$$\beta/2a \,.\, \Sigma q V \mathbf{s}\mathbf{v} = \mathbf{M} = \int \mathbf{I} d\tau.$$

The first term represents theory, derived by analogy from experiments on ordinary metallic circuits.[26] An excellent theory; but we cannot measure the individual items (q, $\mathbf{s}$, $\mathbf{v}$). The second term (magnetic moment per meso-volume) may be regarded as directly measurable. The third term represents the process we have called 'mathematical continuisation'; its object is simply to help our analytical reasoning. On the one side of our measurement we have a statistical physical theory, on the other side an analytical process which is often confounded with theory. But to get either we must—as in the famous recipe for hare-soup—start with our magnet. And to deal quantitatively with magnetic phenomena we must adopt magnetic units. The question of units is entirely practical, it has no repercussions on theory. The plain fact is that magnetic measurements are prior to and independent of electromagnetic observations. We cannot possibly

[26] There is of course the further serious difficulty that the electron itself must in certain cases be treated as a magnetic doublet.

pretend that, in macroscopic laboratory measurements, we have got beyond the use of permanent magnets or that we can actually observe the Amperian whirls which we theoretically postulate.

6. Homometric Systems.

We require an adjective which is more general than 'similar' but includes it as a particular case. We propose to use the word 'homometric.' Homometric systems are those in which ratios such as L, M, T are not necessarily the same constants throughout, i.e. for all corresponding parts, but have several values assigned to them. For example, instead of making the mass-ratios of *all* the corresponding pairs of particles equal to M, we might take the ratio for one pair to be M_1, that for another pair M_2. (i) As an illustration consider the two following systems: (1) a planet m_1 moving in its orbit round the sun m', (2) a planet m_2 moving round the sun m'. The two systems are not dynamically similar; $M = m_1/m_2$ and $M' = m'/m' = 1$ are not equal. Yet it is quite easy to deduce Kepler's third law from logometric considerations based on our second interpretation of measure-ratios. From the law of attraction $f = \gamma mm'/r^2$ we have at once

$$ML/T^2 = F = [\gamma]MM'/L^2 = M/L^2,$$

since $[\gamma] = M' = 1$. Hence $L^3 = T^2$, or a^3 varies as t^2.

(ii) Again, consider the following problem [27]: To formulate the dependence of the period (t) of a pendulum on its length (l), its distance (r) from the centre of the earth, and m the mass of the earth. The motion of the pendulum is given by $d^2x/dt^2 + (g/l)x = 0$, so that $G = L/T^2$. But g itself varies according to the law $g = \gamma m/r^2$, so that $G = M/R^2$. Hence $T = RL^{\frac{1}{2}}/M^{\frac{1}{2}}$, or t varies as $r\sqrt{l/m}$. It will be observed that we have taken two different length-ratios: L for pendulum-dimensions and R for the distances from the earth's centre.

(iii) It is often useful to take X as the length-ratio in one direction and Y as the length-ratio in a perpendicular direction. For example, let us find how the flow over a rectangular notch (in which the correction for end contraction is negligible) varies

[27] 'Dimensional analysis cannot attack this problem at all.'—Mrs. T. Ehrenfest-Afanassjewa, PM 1 (1926) 271. It all depends on what one means by 'dimensional analysis'! If we had taken R and L to be equal we should have found $t = m^{-\frac{1}{2}}l^{\frac{3}{2}}f(r/l)$.

with the head (h) and the breadth (b). Calling vertical distances y and horizontal distances x, we have $1 = G = Y/T^2 = V^2/Y$. And since $q = xyv$, $Q = XYV = XY^{3/2}$. Hence q varies as $bh^{3/2}$.

(iv) As an important practical application consider the following so-called 'law of similitude' [28]:

> If we multiply all the linear dimensions of a vessel by a number L, the periods of the oscillations are multiplied by $L^{\frac{1}{2}}$. . . . If for a shallow basin we multiply the horizontal dimensions by X and the vertical dimensions by Y, the periods of the oscillations are multiplied by $X/Y^{\frac{1}{2}}$. In this new form the theorem of similitude is successfully applicable to the experimental study of seiches with reduced models.

It is often important to study, by means of a reduced model, the action of the tidal ebb and flow of river water in an estuary on the formation of shoals. Owing to the great difference between the horizontal distances and the vertical heights involved in an estuary, it is practically impossible to make a dynamically similar model. If we did so, however, the law would be quite simple. Let L be the scale-ratio and let T be the ratio of corresponding durations (e.g. the time interval between successive high tides). Then, since g is the same for both systems,

$$1 = G = L/T^2.$$

Therefore $T = L^{\frac{1}{2}}$, which is the required formula. Suppose now that we use a much larger scale-ratio (Y) for vertical heights than that (X) for horizontal distances. The systems are now homometric, but not similar. We can obtain in various ways the required modification in the formula. The following is about the simplest.[29] At the surface $g = \omega^2 r$; the surface being flat the radius of curvature r is given very approximately by $x^2 = 2ry$. Hence we have

$$1 = G = R/T^2, \quad R = X^2/Y.$$

That is, $T = X/Y^{\frac{1}{2}}$.

[28] H. Vergne, *Ondes liquides de gravité*, 1928, p. 53 f. The formula $T = X/Y^{\frac{1}{2}}$ is also given in E. Fichot, *Les marées*, 1923, p. 102; it has been applied to models of bays and of seiches in lakes—*cf.* Honda and others, *J. Coll. Sci. Tokyo*, 24 (1908) 52 f. Gibson (*Proc. Inst. Mech. Eng.*, 1924, p. 61) gives the formula without proof and says it is 'necessary for dynamical similarity.' The whole point is that the systems are homometric but *not* similar.

[29] Otherwise: If f is the velocity-potential, $v = \nabla f$ and $gy = \partial f/\partial t$. Neglecting the vertical velocity, we have $X/T = V = F/X$; also $GY = F/T$. Whence $T = X/Y^{\frac{1}{2}}$. According to Gibson, usual scales for models of tidal estuaries are $X = 1/8000$, $Y = 1/200$.—*Nature*, 133 (1934) 969.

(v) In the same way we can deal with the 'specific speed' (s) of a turbine, i.e. the speed (in revs. per min.) at which a turbine would operate if reduced geometrically to such a size that it would develop one horse-power—per jet or runner if there are several—under unit working head.[30] A turbine and its model are homometric but not dynamically similar. Let $H = h/h'$ be the ratio of the heads, $A = a/a'$ the ratio of the wheel radii, $B = b/b'$ the ratio of the jet radii. If v is the peripheral speed for maximum efficiency and u the jet-speed, then v/u is constant for a given type of machine. We have the following equations :

(a) $u^2 = 2gh$, hence $U^2 = H$.

(b) $2\pi an/60 = v = \text{const. } u$, where n is in r.p.m. ; hence $NA = U$.

(c) Horse-power per jet $=$ constant $\times$ area $\times\, u^3$, or $p \propto b^2u^3$; hence $P = B^2U^3$.

It follows that

$$NP^{\frac{1}{2}}H^{-\frac{5}{4}} = B/A = 1,$$

if the turbine and model are geometrically similar ($A = B = L$). By definition $n' = s$ when $p' = h' = 1$. Therefore

$$s = np^{\frac{1}{2}}h^{-\frac{5}{4}}.$$

As an example take a Pelton wheel (hydraulic efficiency $0{\cdot}85$).

$$v = 0{\cdot}46u$$
$$550\, p = 0{\cdot}85\, w\pi b^2u^3/2g.$$

If $b/a = 1/12$, s is found to be $4{\cdot}6$. (For modern mixed flow turbines $s > 100$.) For example, what is the speed of a single-jet Pelton wheel for an output of 2500 b.h.p. under a head of 900 feet ? The answer is

$$n = 4{\cdot}6\, p^{-\frac{1}{2}}h^{\frac{5}{4}}$$
$$= 455 \text{ r.p.m.}$$

(vi) One more example will be given in order to illustrate the point that a homometric transformation is most useful when we know the differential equation involved in the problem. Suppose we want to find the greatest height of a uniform cylindrical vertical pole, consistent with stability, i.e. if carried to a greater height

[30] See for example Gibson, *Hydraulics*, 1925[3], p. 521 : Lea, *Hydraulics*, 1905[5], p. 534. Usually the friction is relatively greater and the design of the setting less accurate in the model.

it will curve under its own weight if ever so slightly displaced—in fact, like a cat's tail or a wheat-straw. It is easy to prove [31] the following equation :

$$EIy_3 = -wAxy_1,$$

where E is Young's modulus, $I = Ak^2$ is the second-moment of the cross-section about the neutral axis, w is the weight per unit length, x is the vertical depth of the section, y is the (horizontal) deflection and the suffixes denote differentiation with respect to x. Consider a homometric system ($X = Z$, Y). The equation gives us at once

$$[Ek^2]Y/X^3 = [w]XY/X,$$

or

$$X^3 = [Ek^2/w].$$

Now obviously the heights at which instability sets in are corresponding heights in the two systems. Hence the required answer is

$$h = C(Ek^2/w)^{\frac{1}{3}}.$$

If we solved the differential equation directly, we should find the 'operational' constant to be

$$C = [9 \times (1\cdot 88)^2/4]^{\frac{1}{3}} = 1\cdot 996.$$

But this would require a knowledge of Bessel's functions. Our use of the simple, almost automatic, homometric transformation may accordingly be described in this case as a neat dodge for avoiding difficult mathematics.

(*a*) Let us now apply these elementary principles to some electrical problems. First consider the high-vacuum current law, the variation of thermionic current-density with potential.[32]

Let $u = \rho v$ be the thermionic current-density, V the potential-difference between the hot and cold electrodes, which are plane-parallel at a distance a, $-q$ and m the charge and mass of an

[31] Consider the equilibrium of the portion between the heights $h - x$ and h. The weight wAx is balanced by the component $F \sin\theta$ of the shear F. We have $F = -EIy_3$ and $\sin\theta \to \tan\theta = y_1$. Whence the equation follows. The direct solution is given by Greenhill, *Proc. Camb. Phil. Soc.* 4 (1881) 67.

[32] Child, PR 32 (1911) 498 ; Langmuir, PR 2 (1913) 453 ; Langmuir and Blodgett, PR 24 (1924) 49 ; J. J. and G. P. Thomson, i. 373 ; K. Emeléus, *Conduction of Electricity through Gases*, 1929, p. 42.

electron. Denote by x distances perpendicular to the electrodes and by y distances parallel to them. Then we have :

(1) $D \equiv [\rho] = Q/XY^2$, so that $U = DX/T = Q/Y^2T$.

(2) $d^2V/dx^2 = 4\pi\rho$, so that $[V]/X^2 = Q/XY^2$, or $[V] = QX/Y^2$.

(3) $\frac{1}{2}mv^2 = qV$, neglecting the initial electronic velocities, so that $MX^2/T^2 = Q[V]$.

Hence

$$U^2 = [V^3]Q/MX^4$$

or

$$u = C(q/m)^{\frac{1}{2}}V^{\frac{3}{2}}/a^2, \tag{15.9}$$

where C is shown *aliunde* to be $\sqrt{2}/9\pi$.

Similar considerations apply to the cylindrical field between an inner kathode (radius b) and an outer anode (radius a). The expression for u must then be multiplied by the symmetric function $\varphi(a/b)$. It can be proved that

$$\varphi^{\frac{3}{2}} = p - 2p^2/5 + 11p^3/120 + \ldots,$$

where p is $\ln(a/b)$.

Referring to equation (15.9), J. J. and G. P. Thomson make the following remark (ii. 426*) :

> This is obtained on the supposition that there is no ionisation between the electrodes, and so does not apply to the cathode fall of potential. The result $ua^2 = \text{const.} \times V^{\frac{3}{2}}$ is however not limited to the conditions postulated in the space-charge equation ; it holds for example when there is uniform ionisation. It follows from the method of dimensions that $V^{\frac{3}{2}}$ is of the same dimensions as $ua^2/(m/q)^{\frac{1}{2}}$, so that the ratio must be of no dimensions ; and though we can find other combinations such as q^4/mu^2a^7 which are of no dimensions, the simplest assumption is that the ratio is a numerical constant.

The reference to other non-dimensional combinations is incorrect. It is due to the misuse of 'dimensions,' i.e. confusion of the two meanings of measure-ratio. The present problem has nothing to do with changes of units.

(*b*) We can also consider a stationary electric current in an ionised gas.[33] Let us use the following notation :

n_1, n_2 = numbers of positive and negative ions per unit volume at the position x.

[33] *Cf.* H. Seemann, AP 38 (1912) 781 ; J. J. and G. P. Thomson, i. 193. Mache (i. 43) and Knaffl (p. 45) talk of applying 'Reynolds's treatment of similarity.' But the case has nothing to do with similarity.

q = number of positive or negative ions produced in unit time per unit volume at this point by the ionising agent.

E = electric intensity at this point.

v_1, v_2 = the velocities of the positive and negative ions under unit electric intensity.

e = charge on an ion.

Then

(1) $$\text{div } \mathbf{E} = dE/dx = 4\pi e(n_1 - n_2).$$

Also, neglecting any motion of the ions except that caused by the electric field, the current through unit area of the gas is given by

(2) $$u = (v_1 n_1 + v_2 n_2)eE.$$

In the steady state, n_1 and n_2 are constant, i.e. losses are balanced by gains. The number of collisions in unit volume being proportional to $n_1 n_2$, we assume that the number of positive or negative ions recombining per unit volume is $\alpha n_1 n_2$. (Over a wide range it is found that the specific mobilities (v_1, v_2) are inversely proportional to the pressure, and α is directly proportional to the pressure.) Owing to the motion of the ions under the electric force, positive ions are being lost at the rate $d/dx \,.\, (n_1 v_1 E)$ and negative ones at the rate $-\,d/dx \,.\, (n_2 v_2 E)$. We neglect diffusion which, except for very weak fields, is relatively insignificant. Since q is the rate of gain owing to ionisation, we have

(3) $$q - \alpha n_1 n_2 = d/dx \,.\, (n_1 v_1 E) = -\,d/dx \,.\, (n_2 v_2 E).$$

We thus have three equations connected with the ionic current in the gas. If we keep the kind of gas, the pressure and the temperature unaltered, then α, e, v_1, v_2 remain constant. Let us see how the other quantities vary. We have

(2) $$U = N[E] = N[V]/L.$$

(3) $$Q = N^2 = N[E]/L = N[V]/L^2.$$

Whence

$$[V] = Q^{\frac{1}{2}}L^2$$
$$U = QL = [V^2]/L^3.$$

Suppose the electrode is bounded by a finite surface perpendicular to the streamlines (e.g. similar spherical condensers), the total current is $j = \int u dS$ or $J = L^2 U$. Suppose the streamlines are in parallel planes (e.g. a very long cylindrical condenser),

then the current per unit length is $j = \int u ds$ or $J = LU$. Suppose the streamlines are parallel (e.g. practically in a plate-condenser), then the current per unit surface is $j = u$. The following results have been approximately verified. (1) The same condenser ($L = 1$) with different voltages, $J = [V^2]$ or $j_1/j_2 = V_1^2/V_2^2$. (2) The same ionising intensity ($Q = 1$) with different plate condensers, $J = L$ or $j_1/j_2 = l_1/l_2$.

(c) Townsend has enunciated the following 'general theorem relating to the sparking potentials':

> If V be the potential difference required to produce a discharge through a gas at pressure p between two conductors A and B, the same potential difference will produce a discharge through a gas at a lower pressure $p' = p/L$ between two conductors A' and B' of the same shape and in the same relative position, but with all the linear dimensions increased in size so that the distance between points on A' and B' exceeds the distance between the corresponding points on A and B in the ratio $L/1$.—J. S. Townsend, *Electricity in Gases*, 1915, p. 365*. (We have altered the notation.) Cf. Townsend, *Electrician*, 71 (1913) 348.

The Thomsons (ii. 531) call this a 'general similarity relation' applying to all cases of discharge depending on ionisation by collision, whatever be the shape of the electrodes. The systems are, in our phrase, homometric; though geometrically similar, they are not dynamically or physically similar. The transformation is as follows:

(1) $$P = 1/L$$

(2) $$[V] = 1, \text{ and hence } [E] = 1/L.$$

When conductivity is produced by X-rays or by ultra-violet light, the number (n) of ions produced by collisions by an ion per cm. of its path—or briefly the number of collisions per cm.—depends on the velocity with which the ion collides with a molecule, i.e. on $E\lambda$, where λ is the free path; it is also directly proportional to the pressure.[34] So we can take

$$n = pf(E/p).$$

According to Townsend (p. 294) this function approximates to $a \exp(-bp/E)$. For the two systems envisaged $[n] = 1/L$ or $[nl] = 1$, i.e. the number of ions produced between corresponding

[34] The velocity is $v = (2q\phi/m)^{\frac{1}{2}}$. At low pressures the velocity depends on the potential difference ($\phi = E\lambda$) and at high pressures on the field; in either case $[v] = 1$. The density of the gas $\propto 1/\lambda$; hence at the same temperature $P = 1/L$.

points is the same. Hence if the conditions are such that a spark is on the point of passing in one case, it will be so in the other. The relation still holds even if part of the ionisation is due to impact of the ions on the electrodes, for the energy with which they strike depends only on E/p.

Since the velocity of an ion is $v = (2qV/m)^{\frac{1}{2}}$, and q, V, m are the same for both systems, $[v] = 1$. Also $4\pi\rho = -\nabla^2 V$, hence $[\rho] = 1/L^2$. Hence the ratio of current-intensities is

$$U = [\rho v] = 1/L^2$$

and the total currents are the same ($J = 1$). In accordance with this, it has been found [35] that the abnormal cathode fall is given by the formula

$$V = au^{\frac{1}{2}}/p + b,$$

where a and b are constants depending on the gas ; and the thickness of the dark space is

$$d = A/p + B/u^{\frac{1}{2}}.$$

(*d*) Assume [36] that for the positive column in low-pressure

$$E = f(\lambda, r)$$

discharge where λ is the mean free path and r is the radius of the tube. Then for another tube related by Townsend's transformation

$$E/L = f(L\lambda, Lr),$$

so that

$$f(L\lambda, Lr) = L^{-1}f(\lambda, r)$$

for all values of L. That is,

$$f(\lambda, r) = \lambda^{-1}\varphi(\lambda/r).$$

Güntherschulze found

$$E = C\lambda^{-1}(\lambda/r)^a,$$

where for N_2 and H_2, $a = 1/3$; and $a = 1$ for Ne.

In discharges [37] such as the arc or later stages of the spark, thermal ionisation is important. The heat (h) generated in

[35] Aston, PRS 79 (1907) 80 ; Aston and Watson, PRS 86 (1911) 168.

[36] A. Güntherschulze, ZfP 41 (1927) 718. *Cf.* R. Holm, *ibid.* 75 (1932) 171.

[37] J. J. and G. P. Thomson, ii. 598. G. Heller has an article on 'Dynamical Similarity Laws of the Mercury High Pressure Discharge' in *Physics*, 6 (1935), 389–394. It would take too long to consider the article here. His conclusions may be correct, but his argument is invalid as he does not correctly apply his homometric transformation.

corresponding volumes is proportional to j or ul^2, hence $[h] = 1$. Not so for the heat lost ; for the loss by conduction $\propto$ (area $\times$ temp. gradient) $\propto l$, for the same temperature distribution ; conductivity does not depend appreciably on temperature. Hence if one system is in thermal equilibrium, the other cannot be so unless it has a different temperature-distribution.

We have now shown that the simple and elementary idea of measure-ratios, in its two-fold interpretation, not only gets rid of all these dimensional pseudo-problems which have for so long been a bugbear to students and an obsession for technologists, but that it also is a very useful expedient in many practical problems which occur in physics as well as in civil and electrical engineering.

7. What is Electricity ?

In order to deal with this inevitable question, let us revert to a notation introduced in the last chapter. There we used words with capital letters, such as Length and Time, to *designate* or refer to magnitudes whose ratios constitute our basic measures. Let us now extend this usage and call Electricity the entity—almost certainly consisting of discrete things called electrons—whose presence causes electrical phenomena. In other words, Electricity refers to the objective context of our equations, these latter containing only electricity (without a capital letter) or charge, i.e. the number q.

Let us begin with Coulomb's law $f = qq'/\alpha r^2$, which defines charge as a ' physical quantity ' or measure in terms of prior measures (f and r) and an arbitrary number (α). Coulomb was undoubtedly helped by the analogy of Newton's law of gravitation. The force between two small charged conducting spheres was measured on the torsion balance. A was then withdrawn and brought into contact with a third sphere C of the same dimensions but uncharged. By symmetry it was concluded that A and C must on contact have received equal charges. On replacing A on the torsion balance, it was found that the force was halved. It will be observed that in this explanation we are using the word ' charge ' in an ontological sense, to denote something which alters its spatial distribution between two spherical surfaces. Whereas in the end we use the word to denote the number q, an arbitrary number (owing to the presence of the

factor α). It would be pedantic to object to this flexibility of language, but it must not be allowed to obliterate an important distinction of meaning.

The distinction may seem elementary; but, owing to the prevalent misinterpretation of the symbols of physics, it is often overlooked. Maxwell in the following passage, for instance, is surely guilty of confusion.

> While admitting electricity, as we have now done, to the rank of a physical quantity, we must not too hastily assume that it is or is not a substance; or that it is or is not a form of energy; or that it belongs to any known category of physical quantities. . . . The quantities 'electricity' and 'potential' when multiplied together produce the quantity 'energy.' It is impossible therefore that electricity and energy should be quantities of the same category. . . . In most theories on the subject, electricity is treated as a substance. . . . . The use of the word 'fluid' has been apt to deceive the vulgar, including many men of science who are not natural philosophers. . . . For my own part, I look for additional light on the nature of electricity from a study of what takes place in the space intervening between the electrified bodies.—Maxwell, i. 38–43.

The number q which occurs in our formulae is, like other measures, called a physical quantity. It is none the less a pure number, a 'derived quantity.' It is difficult to conceive what more could be known about this number beyond its exact metrical definition. It is defined differently from the number known as potential; and when we change our units, these two measures alter in different ratios. We cannot seriously hold that q is a 'substance'; nor can we with Pohl (p. 32) speak of the 'measurement of electric substance.' There is an element of truth therefore in Eddington's assertion that

> in the scientific world the conception of substance is wholly lacking, and that which most nearly replaces it—viz. electric charge—is not exalted above the other entities of physics.—*The Nature of the Physical World*, 1928, p. 274.

But only in this sense that physics is exclusively concerned with numbers or ratios, these are its 'entities.' Electric charge, the measure q, does not 'replace' the concept of substance, it has not the remotest connection with it.

There is, in fact, no new concept whatever involved in the measure known as 'charge.' Bridgman's contention is unsustainable:

> The measurements involved in these operations are measurements of ordinary mechanical forces. . . . This of course is all very trite; the important thing for us is merely that magnitude of charge or quantity of electricity is an independent physical concept and that unique operations exist for determining it. . . . The operations by which the inverse square law and the concept of the field are established presuppose that the charge is given as an independent concept, since the operations involve a knowledge of charges.—Bridgman, i. 132 f.

Unless Prof. Bridgman is using language in some special sense of his own, he seems to refute himself in the first sentence of this passage. For if the measurements involved are those of ordinary mechanics, how can q—defined and ascertained by such measurements—involve any specific new meaning, category or concept ? The operation of measuring q does not presuppose q; it presupposes ordinary phenomena—mechanical forces, etc. —and nothing else. Electrical science is ultimately based on the addition of q to the symbols of mechanics ; and q is a number defined in terms of mechanical measures.

This simple view disposes of Sir Arthur Eddington's curious theory of 'the cycle of physics, where we run round and round like a kitten chasing its tail and never reach the world-stuff at all.'

> Electric force is defined as something which causes motion of an electric charge; an electric charge is something which exerts electric force. So that an electric charge is something that exerts something that produces motion of something that exerts something that produces . . . ad infinitum.—*Nature of the Physical World*, 1928, p. 264.

What exactly is he trying to 'define' ? If it is the measure q, he should inspect Coulomb's formula which involves no circularity. These quantitative formulations are the only definitions used in physics. Beyond these, of course, there are the operational instructions, the pragmatic laboratory guidance providing the appropriate context for each of these measure-numbers. Electric force is the measure f, independently ascertainable, which occurs in Coulomb's law and its extension by Liénard or Ritz ; electric charge is the q or q' occurring in that formula. Of course there is something, which moves and causes motion, which produces and exerts, etc. This is the language of the laboratory, of ordinary experience. Whether we thus 'reach the world-stuff,' is a problem which may be left to the philosophers ;

the lab-man is too busy, or ought to be, with his job. The essential point is that in practical physics we are engaged in the workaday operational world, and in theoretical physics we are working with numbers. No manipulation of the number q or of the measure-ratio Q can throw any light on 'world-stuff' or on 'the nature of electricity.'

Physics as a science has nothing to do with electricity as a 'substance,' it has just as little to do with it as a 'quality.'

> The quality in virtue of which a body exerts the peculiar force described is called electricity, and its quantity is measured (*ceteris paribus*) by measuring force.—Maxwell-Jenkin, p. 66.
>
> The *charge* of the electron—as well as its mass in the ordinary meaning of classical mechanics—is not matter but a *quality* (*Eigenschaft*). But since this quality is invariant as well as additive (in combinations of several electrons) there ensues the possibility of treating this quality as *representing* the corresponding things.—Frenkel, i. 246.

Such contentions are due to the current misinterpretations of the scope and symbols of physics.

This answer does not in the least deny that there are problems of metaphysics and epistemology; it is merely contended that physics can throw no light on them. Still less, of course, is it suggested that there is nothing beyond measure-numbers or pointer-readings. We have in fact already argued against this paradox. We speak of electricity being located, of charges moving, and so on; and we are perfectly justified in so speaking. We are thus describing phenomena not only occurring in the laboratory but accessible to ordinary experience. Moreover, there is every reason to believe that Electricity, in this simple ontological sense of practical life, is discrete, that it exists as electrons—using the word to include positive and negative particles. But the electron cannot be said to *be* its charge—or even to *have* a charge—if we are using the word 'charge' in the *strict* sense of the measure or ratio we call q. There is here an unavoidable flexibility of language which is liable to misinterpretation. Prof. Millikan tries to clear up the confusion.

> To remove the ambiguity in the definition of the term 'electron' existing at the present time because of the double sense in which it is used in the literature—namely to denote on the one hand . . . *the magnitude of the elementary quantity of electric charge*, and on the other hand the name of a *particle* of a particular mass—the terms 'negatron' and 'positron' are here used. These terms are used

merely as convenient contractions for the fully descriptive *particle* designations : 'free negative electron' and 'free positive electron.' The term electron then retains its historical, derivative and logical meaning as the name of the elementary unit of charge ; and the present ambiguity no longer remains.—Millikan, iii. 332.

To most people this will seem merely to increase the confusion. Is it not sufficient to use electron in the ontological sense and electron-charge in the metrical sense ?

We can now see what are the two possible answers to the question, What is electricity ? And to show that there is no novelty in our view we shall quote two other replies :

Some readers may expect me at this stage to tell them what electricity 'really is.' The fact is that I have already said what it is. It is not a thing, like St. Paul's Cathedral ; it is a way in which things behave. When we have told how things behave when they are electrified and under what circumstances they are electrified, we have told all there is to tell. . . . When I say that an electron has a certain amount of negative electricity, I mean merely that it behaves in a certain way. Electricity is not like red paint, a substance which can be put on to the electron and taken off again ; it is merely a convenient name for certain physical laws.—B. Russell, *ABC of Atoms*, 1924², p. 31 f.

We may, at this stage of our inquiry, try to deal (however inadequately) with the oft-propounded and inevitable question : What is electricity ? . . . The special properties of the atomic fragments give rise to phenomena which we find it convenient to call 'electric' phenomena. . . . We can if we like . . . call the atomic fragments themselves particles of positive or negative electricity as the case may be ; or we may speak of them as particles charged with positive or negative electricity—that is little more than a matter of taste. We know nothing of their ultimate constitution ; and our ignorance in this respect is never likely to be dissipated—and I am afraid I can say no more in answer to the question, What is electricity ?—L. Southerns, *Electricity and the Structure of Matter*, 1925, p. 122.

The first answer is therefore : Electricity means something whose presence is manifested in such and such phenomena ; this something appears to consist of discrete entities which are localisable and movable.[38] Whether entities which act thus are substantial, is a question for a philosopher. As physicists and practical men, we are interested more in behaviour than in nature, what Electricity does rather than what it is.

[38] 'Classical [or any other !] electrodynamics can give no answer to such questions as : what is electricity, why does electricity come in discrete units, or why does it repel itself ? '—Bridgman, iv. 61.

Our second answer is that electricity is the measure or number q. There does not appear to be any third answer possible.

We must therefore reject such attempts as the following :

> What is the particular mode of motion which constitutes electricity, this becomes the question. That it is some kind of molecular vibration, different from the molecular vibrations which luminous bodies give off, is I presume taken for granted by all who bring to the consideration of the matter a knowledge of recent discoveries.—Herbert Spencer, *Essays*, 3 (1874) 91.
>
> Electricity or electrification of a body is only a designation for the modification which the surface-layers experience because stresses of the surrounding medium terminate there.—Ebert, p. 365.
>
> According to the ideas we have developed, the mechanical actions on the parts of the material system, resulting from the established electromagnetic field, are to be regarded merely as the terminal aspects of a state of stress in the medium (the aether) between the bodies.—Livens, ii. 251.

References to molecular vibrations and to aether-strains no longer satisfy us either as practical men or as physical theorists. On the other hand, the rejection of this view may be based on fallacious grounds, as appears to be the case in the following quotations :

> The modern answer to the question 'What is electricity ?' is that it is 'a fundamental entity of nature.'—Ramsey, p. 11.
>
> Not everyone grasps as yet that electricity is one of the fundamental substances of the world. All matter is electrical ; or to put it a little more vividly but not a whit too strongly, *all matter is electricity*.—Karl Darrow, *The Renaissance of Physics*, 1937, p. 19.
>
> Electricity is one of the fundamental conceptions of physics ; it is absurd to expect to be told that it is a kind of liquid or a known kind of force, when we explain the properties of liquids in terms of electricity and electric force is perhaps the fundamental conception of modern physics. . . . In short, the correct question is, What does electricity ? not, What is electricity ? The former has a definite meaning and can be answered ; the latter is not a fair question in that the questioner does not really formulate his inquiry in such a way as to convey what he wants to know. If he means 'Can you express what you know about electricity in terms of something more fundamental ?' the answer is definitely, No. We must have in physics something behind which we do not go ; if it were not electricity, it would have to be some other conception. . . . A race of men is in fact arising who think of the electrical quantities as fundamental and familiar and explain the mechanical quantities, which seem more familiar to most of us, in terms of them.—E. Andrade, *The Mechanism of Nature*, 1930, pp. 15 f., 61.

There is here a grave confusion between the ontological level of experience and philosophy and the mensurational level of physics. If by Electricity we mean the objective agency or thing which causes and enters into certain happenings, then it was never true that we could *define* it; we can only designate it, point it out, appeal to experience. And obviously we cannot explain everything else in terms of it, unless by explanation we mean explaining away. But if, as we should when discussing scientific physics, mean by electricity merely charge, i.e. the number q, then it is clearly untrue to say that q is something behind which we do not go in physics. For q is *defined* in terms of measure-numbers whose priority is assumed. If there are electricians who are so familiar with other quantities based on q that they forget the logical priority of other measure-numbers, their delusion is merely a fault of perspective and a fallacy of habit. There is no objection whatever to explaining the properties of liquids in terms of electricity, just as we have reduced $\varkappa$ and μ to statistical characters of aggregates of electrons. But surely it is quite impossible for electric intensity to be 'the fundamental conception of modern physics.' As we have already explained, **E** is purely an auxiliary mathematical quantity, and its very definition assumes the prior determination of force and charge. Einstein tells us [39] that theoretical physicists 'gradually accustomed themselves to admitting electric and magnetic force as fundamental concepts side by side with those of mechanics, without requiring a mechanical interpretation for them,' so that 'the purely mechanical view of nature was gradually abandoned.' But this recent tendency in speculation relies for its success on a covert appeal against the defective philosophy known as mechanicism. As a statement of the logical position of physics in its proper sphere, it is without foundation, as our previous exposition of the electron theory has shown. If the physical theorist kept more in touch with the instrument-maker and the laboratory-worker, he would not be so prone to exalt derived quantities into 'fundamental concepts.' An increasing amount of mysticism (in the pejorative sense) has of late years been invading physical theory. The space devoted to this chapter, which is practically an excursus on elementary mensuration, has not been employed in vain if it convinces the reader that the symbols of physics are measure-numbers which

[39] *The Meaning of Relativity*, 1922, p. 8.

must be discovered in the laboratory by dealing with the Length and Time of common sense; which, so far from contradicting everyday experience, actually presuppose it as the necessary context which alone gives significance to our algebra. We shall then be in a position to take a much-needed attitude of criticism towards those rather sweeping assertions in which contemporary physicists are apt to indulge.

> Matter is just electricity and nothing else. . . . We only know of electricity in the form of electrons and protons, so that it is meaningless to speak of these indivisible particles as if they consisted of two parts: electricity and matter.—H. A. Wilson, *The Mysteries of the Atom*, 1934.
>
> Kaufmann's experiments show that the real constant mass of the electron is negligible compared with the apparent mass; it can be considered as zero, so that if it is mass which constitutes matter we can almost say that matter no longer exists. . . . One might say: There are merely holes in the aether.—Poincaré, *Revue scientifique*, 7 Aug. 1909, p. 174.
>
> In the present state of science one may admit the existence of pure electric charges, positive and negative, independent of any material support.—Jouguet, p. 3.
>
> Matter, the substratum of mechanical phenomena, is explained by starting from electricity.—L. Rougier, *Philosophy and the New Physics*, p. 58.
>
> It is a necessary consequence that matter has no existence of its own, it represents rather only a special appearance-form of aether-states. This statement is the quintessence of both theories of relativity.—G. Mie, p. 421.
>
> In the modern system of physics electricity no longer stands alongside of matter, it has taken the place of matter. . . . The conception of matter lost its original meaning as a result of the electron theory.—Haas, *The New Physics*, 1923, pp. 71, 123.
>
> Recent physical speculation . . . dispenses entirely with aether and matter as independent entities, and regards energy as the one fundamental quantity with which physical science deals.—Livens, ii. 237.

In so far as a term such as electricity has a reference beyond being a generalised algebraic number, it assumes the ordinary world as known in the laboratory; and one of the obvious ingredients of such a world is what we know as Matter. It is simply ludicrous to tell us that these numbers which we have thus constructed enable us to dispense entirely with the essential constituents of the real world which alone confers on these numbers a significance beyond that of abstract arithmetic. It is not true, as we are complacently informed, that

> the physicist has been striving for years to attach a clear meaning to the term *matter*, and undoubtedly we have reason to believe that the concept means much more to us to-day than to the physicists of fifty years ago.—Lindsay-Margenau, *Foundations of Physics*, 1936, p. 2.

The practical physicist takes Matter in the laboratory, just as he eats his dinner, exactly as ordinary mortals do. The theoretical physicist is dealing altogether with numbers such as mass, charge, etc. About the structural characteristics within the world of experience, we certainly know a great deal more than unaided sensation could tell us. But physics has not contributed one iota towards modifying or clarifying our ideas of Matter and Electricity. Physicists are only bluffing when they pretend otherwise.

8. Epilogue.

Apart from the mathematical conundrums found in more advanced text-books and the technical and practical examples found in others, we have now traversed the domain of classical electromagnetics, with the deliberate omission of thermo-electric and allied phenomena. Certain difficulties have been encountered and no attempt has been made to gloss over them While the primary aim of this book has been to give a logical and synthetic presentation of electromagnetic theory, it has also been found necessary to revise accepted views of its history. But this revision, which will evoke opposition from those who profess to be—but are not really—followers of Maxwell, is quite subordinate to the main object of a logical exposition.[40] A brief summary of the principal conclusions will now be given.

(1) It is impossible to consider electromagnetics apart from certain wider issues raised by physical science. We must make up our minds concerning the origin, validity and meaning of the symbols of physics. This fundamental question does not usually arise at the beginning of the development of a science, but rather

[40] The exaggerated cult of Maxwell has been amply illustrated in the foregoing pages. Here is another typical outburst: 'His electromagnetic theory of light seemed to his followers not only an interesting scientific advance but, in the quality of its originality, something of a superhuman revelation. Maxwell is the first man whose work takes us outside the Newtonian scheme.'—J. W. N. Sullivan, *Contemporary Mind*, 1934, p. 73 f.

in its later stages. An interesting parallel may be cited from the science of economics :

> We all talk about the same things, but we are not yet agreed what it is we are talking about. . . . It is fundamentally important to distinguish between the actual practice of economists and the logic which it implies, and their occasional *ex post facto* apologia. . . . The propositions of economics . . . are deductions from simple assumptions reflecting very elementary facts of general experience. —Prof. Lionel Robbins, *Essay on the Nature and Significance of Economic Science*, 1937[2]; pp. 1, 85, 104.

Physicists all talk about the same things, but they are not yet agreed what it is they are talking about. Witness the collecting of international votes on the dimensions of B and H, the alleged degradation of Space and Time to shadows, the pretended measurement of mass in kilometres, the reduction of everything (elephants included) to pointer-readings. Any faddist or group with a particular philosophy at present finds physics a happy hunting-ground. Every popular or ostensibly philosophical book on modern physics [41] is a farrago of materialism or idealism or some new-fangled 'ism, dogmatically propounded with the prestige of 'science.' The ill-educated layman succumbs to the propaganda ; most professional philosophers, overawed by elementary algebra, try to convert the stuff into grist for their particular mill. The others, reviving the Averroist hypothesis of two truths, admit that it may be all right in science, but hold that it is wrong in philosophy.

In spite of the general impression to the contrary, what has been lacking is the *internal* criticism of physics. There has merely been a variegated invasion : mathematicians fresh from the logical analysis of geometry and hungry for new systems of postulates, idealists anxious to tickle the bourgeois with paradoxes, pragmatists desirous of emulating in physics the achievements of the behaviourists in psychology.

Against this alien intrusion it has here been maintained that the symbols of physics represent nothing but numbers, that they throw no light whatever on any philosophical problem, that their genesis and verification lie in the laboratory at the level of everyday unsophisticated experience. The ultimate in physics

[41] 'Every scientist turned philosopher tends to find support in his special studies for the metaphysical theory which *on other grounds* he finds attractive.' —Stebbing, *Philosophy and the Physicists*, 1937, p. 283.

is not based on a noumenal or ghostly 'scientific world'; it is founded on the *argumentum ad lab.* So far as electromagnetics is directly or indirectly concerned, all these imposed irrelevant elements—relativity, four-fold world, dimensions, coincidences, pointer-readings, operations, imaginary observers and clocks, local time—have been rebutted and rejected in the foregoing pages. The philosophical neutrality and the pragmatic character of physics has been upheld. This conclusion is, of course, opposed to all the recent attempts to foist a particular brand of philosophy into scientific physics: the Berkeleyianism of Sir James Jeans, the symbolism with a background of mental activity upheld by Sir Arthur Eddington, Einstein's subjectivist theory of Space and Duration, Prof. Dingle's solipsism,[42] the logical positivism of the Vienna School.

(2) The exposition of electromagnetic theory must nowadays be adjusted to the view that the entities concerned in the phenomena are discontinuous, that the laws involved are statistical results due to the interaction of a great number of these 'electrical molecules.' This adjustment has not yet been effected in the text-books. Laws of force between metallic circuits, induction, Poynting's theorem, etc., are usually catapulted one by one at the student, with little or no interconnection and without reference to the electron theory. The proofs are often eked out with metaphors concerning alleged stresses, 'fields,' and the like. The appropriate theoretical substructure for these various phenomena is a law of force between moving charges. And this law must involve the element of propagation in time. Into this scheme there must also be fitted radiation from charge-complexes.

It must be realised that this idea, which is the basis of the treatment in this book, is quite opposed to the point of view still advocated, e.g. by Einstein:

> This progress has to be paid for by increasing the complexity of the forces of interaction which had to be assumed as existing between electrical masses in motion. The escape from this unsatisfactory situation by the electric field theory of Faraday and

[42] 'Relativity is in fact completely solipsistic.'—H. Dingle, *Philosophy* 11 (1936) 57. This is against Milne (*Relativity, Gravitation and World-Structure*, 1935, p. 16): 'Relativity and solipsism are incompatibles.' On the view maintained in the present book, *both* statements verge on the absurd. It is as if one said, 'a second-order correction to our laboratory-measures is inconsistent with the existence of Canada,' or 'Voigt's algebraic formula implies the objective existence of mutton-chops.'

Maxwell represents probably the most profound transformation which has been experienced by the foundations of physics since Newton's time. . . . The existence of the field manifests itself indeed only when electrically charged bodies are introduced into it. The differential equations of Maxwell connect the spatial and temporal differential coefficients of the electric and magnetic fields. The electric masses are nothing more than places of non-disappearing divergency of the electric field. . . . What appears certain to me, however, [as against Lorentz], is that, in the foundations of any consistent field theory, there shall not be in addition to the concept of field any concept concerning particles. The whole theory must be based solely on partial differential equations and their singularity-free solutions. . . . The field, as determined by differential equations, takes the place of the force.—Einstein, *J. Franklin Inst.*, 221 (1936), 363–5, 367.

The present volume is really a reply to this rather dictatorial pronouncement that the Faraday-Maxwell theory is an 'escape' from the 'unsatisfactory' view of Gauss-Weber. We claim to have shown that—apart from radiation—this idea of 'field' is nothing but an otiose metaphor, and that the partial differential equations must be replaced by a force-law by whose aid we can express the electron theory coordinating all the relevant phenomena.[43]

(3) Now there exists a synthetic formula fulfilling these conditions, namely, the Liénard force-formula. Historically this originated in the retarded potentials of Riemann (1858) and Lorenz (1867), Liénard's paper of 1898, supplemented by Schwarzschild (1903) and Ritz (1908). This is found to give an excellent synthesis, though several difficulties remain, the chief being the mass-velocity law. This formula presupposes velocities which are 'absolute,' in the sense that it involves separately the velocity of each interacting point-charge relative to a framework or schesis. The attempts of relativists to avoid an explicit admission of this fact have led to deplorable confusion and to puerile verbal gymnastics. It has been shown that electromagnetic phenomena require the framework to move with the earth (at least in its orbital motion). The only objection to this

[43] A few pages later (p. 375) Einstein accepts 'Millikan's demonstration of the discrete nature of electricity.' 'The field did not exist for the physicist of the early years of the nineteenth century. . . . He tried to describe the action of two electric charges only by concepts referring directly to the two charges.'—Einstein and Infeld, *The Evolution of Physics*, 1938, p. 157. We not only tried in this book, we think we have actually succeeded. And we do not mind being called early Victorian !

lies in optics, where Lorentz showed inveterate but unjustified prejudice against Stokes's solution of the aberration problem.[44]

(4) There is, however, an alternative formula, that of Ritz (1908), based on the work of Gauss (1835) and Weber (1846). Barring a brief but stupid objection (concerning double stars) sometimes mentioned in text-books of optics, Ritz's formula has been completely boycotted. This is rather unexpected in these piping days of 'relativity,' for the formula involves only the relative velocity of the two interacting charges. But presumably relativity can be carried too far! Nevertheless we have taken Ritz's formula seriously and we have tested it against experimental results. And, surprisingly, we have found that it stands the test better than that of Liénard. In other words, the formula has found the right of domicile in the science of electromagnetics. The orthodox of to-day obviously think otherwise; the *onus probandi* has now been placed upon them. It will be interesting to see whether the conspiracy of silence can be overcome—it would mean the suppression of such a lot of beautiful mathematics, not to speak of brand-new philosophy!

(5) In physics the mathematicians rule the roast to-day; no pedestrian physicist or mere laboratory man dares to stand up to them. Seizing upon an elementary and innocuous transformation given by Voigt in 1887, they have, under the leadership of Einstein (1905), erected a weird structure labelled 'relativity.' It involves the introduction of *dates* into physical laws, the proceeding being skilfully concealed by the invention of imaginary 'clocks.' Apriorism, which we thought defunct in science, is ushered back again: laboratory results are triumphantly deduced from a mathematical transformation of the alleged measurements of a non-existent observer.[45]

[44] We are not here dealing with astronomical phenomena. But of course the schesis for the fixed stars must be different from that which applies to a terrestrial laboratory. Lorentz's objection to Stokes's theory is *not* based on electromagnetics or optics, but on hydrodynamics! That is, it is founded on the superseded idea of an elastic aether. So, with Planck, Lorentz is 'inclined to prefer the unchangeable and immovable ether of Fresnel' (viii. 173). He does not even mention the possibility of a ballistic theory.

[45] A colleague has objected that I should not blame a theory for the irrelevant or foolish statements of its exponents. As Eddington himself would say: 'A doctrine is not to be judged by the follies that have been committed in its name.'—*Nature* 139 (1937) 1000. But expositions of relativity are irremediably permeated with imaginary observers. Also I have not quoted the most absurd of

The real popularity of the new ideas began when Minkowski (1908) adopted the analytical dodge metaphorically called the use of four dimensions. The mathematicians got their chance, and the semi-educated developed their natural gullibility. So to-day everyone with a reputation at stake or with an ambition to be up to date has to be a relativist.

Nevertheless, greatly daring, we have examined and rejected these claims so far as electromagnetics is concerned. Our procedure has been purely scientific; we have employed no philosophical or popular arguments against Einstein's theory. First taking Voigt's formula merely as an analytical manipulation of a kind known elsewhere in physics and geometry, we showed that practically all the results dubbed 'relativist' are algebraic identities, quite independent of the peculiar interpretation which relativists seek to impose subsequently upon what is the common patrimony of all physicists. In other words, the nerve of the reasoning is a succession of analytical commonplaces; it does not at all depend on the picturesque but cloudy *discourse* with which contemporary writers accompany it. Certain apparent first-order results were proved to be derived from simple Newtonian kinematics (subrelativity). The alleged and unverified second-order Doppler effect was shown to depend on a manipulation of dates. Lorentz's mass-velocity formula was admitted to have the sophisticated mathematical property of being 'covariant'; and if anyone thinks that it is thereby *proved*, he cannot be gainsaid even by an experimentalist. Such is the case against 'relativity' in electromagnetics—apart altogether from the arguments in favour of Ritz's formula. It is to be hoped that this purely scientific challenge will be either answered or accepted. But in present-day physics there is much more authoritarian orthodoxy than those inside like to admit or those outside suspect.

(6) Since the present book was written there has appeared in the pages of *Nature* a very timely discussion which was initiated

the statements, e.g. Eddington (p. 59): 'Let us look at a β particle from its own point of view. . . . The β particle, snugly thinking itself at rest, pays no attention to our goings on, and arranges itself with the usual mass, radius and charge.' Says Miss Stebbing of Eddington and Jeans: 'Both these writers approach their task through an emotional fog; they present their views with an amount of personification and metaphor that reduces them to the level of revivalist preachers.' —*Philosophy and the Physicists*, 1937, p. 6.

by Prof. Dingle.[46] He certainly did not mince his words in condemning the apriorism of contemporary physics. He speaks of 'the wholesale publication of spineless rhetoric, the irrationality of which is obscured by a smoke-screen of mathematical symbols.' To one leading physicist he attributes a 'combination of paralysis of the reason with intoxication of the fancy'; concerning another, Dingle asserts that 'he does not want to know about the external world, definitions are all that matter.' And he makes the following plea for a return to commonsense (p. 1012):

> The criterion for distinguishing sense from nonsense has to a large extent been lost; our minds are ready to tolerate any statement, no matter how ridiculous it obviously is, if only it comes from a man of repute and is accompanied by an array of symbols in Clarendon type. If this state of mind exists among the *élite* of science, what will be the state of mind of a public taught to measure the value of an idea in terms of its incomprehensibility and to scorn the old science because it could be understood?

Such language is extremely refreshing and is in obvious agreement with the attitude adopted in the present work. But Prof. Dingle's invective refers chiefly to what he calls 'cosmolatry,' i.e. the misuse of general relativity with which we hope to deal in a subsequent volume. Nor does he attempt the far more difficult task of a much-needed internal criticism of physics, the first instalment of which is now before the reader. Indeed, as the author of a book entitled *Relativity for All* (1922³), he would presumably not go so far as we have done in condemnation of apriorism and algebraicism.[47]

(7) Another expression of reaction against popularised philosophised physics is to be found in a book which appeared while

[46] H. Dingle, 'Modern Aristotelianism.'—*Nature* 139 (1937) 784–6; 'Deductive and Inductive Methods in Science, A Reply,' *ibid.*, p. 1011 f. The discussion is on pp. 997–1010, 1025 f. The pejorative term 'Aristotelianism' is historically absurd; 'Hegelianism' would be much more appropriate. I hope in a subsequent volume to discuss the new physics of Prof. E. A. Milne. Meanwhile it is good to hear Prof. Dingle asking (p. 1012): 'Since when has the Royal Society been dedicated to the study of definitions?'

[47] In his subsequently published *Through Science to Philosophy* (1937), Prof. Dingle says (p. 337) that the logical positivists 'are not describing physics but making *a priori* postulates of their own.' But he himself has his own philosophical axe to grind. And on p. 243 he seriously speaks of 'special relativity eliminating time and general relativity mass.' In *Nature* 141 (1938) 307, he maintains that 'the most essential fact is that modern physics has something vital to say to philosophy,' whereas we think that it has nothing to say.

the proofs of the present work were being corrected : *Philosophy and the Physicists* (1937), by Miss L. Susan Stebbing, Professor of Philosophy in the University of London. The author utters many severe criticisms of Eddington and Jeans, thus corroborating several of the arguments expressed in the foregoing pages.[48] But Prof. Stebbing remains an outsider, respectfully unwilling to query any of the arcana of physics such as relativity or space-time.[49] However—with the possible exception of the relevance of 'indeterminacy' to free-will, a subject we have deferred for subsequent treatment—she seems eventually to reach the position that the new physics is philosophically neutral in the sense that metaphysical arguments remain exactly what they were before Einstein, Bohr or Heisenberg were born.[50] If an expressive slang term be pardoned, we may say that the long overdue process of 'debunking' the claims of contemporary physicists—not physics—seems to have begun.

(8) That this critical reaction must be carried much farther, it is one of the objects of the present book to show. We shall illustrate this need by citing some passages from *The Evolution of Physics*, by Albert Einstein and Leopold Infeld, which has just been published by the Cambridge University Press (1938). The limitation imposed on our subject-matter prevents us from examining here the views which the authors express on optics, general relativity and quantum theory. But two-thirds of the book traverses (without a single equation) ground which has been explored in the present volume ; and there is an extraordinary discrepancy between the two presentations. It is worth while to select a few topics in order to emphasise this contrast.

[48] For instance : the nonsensical denial of solidity (pp. 53, 272) ; Eddington's two tables (55), his symbolic world (65, 127), the elephant and pointer-readings (92), entropy and time's arrow (261).

[49] 'The luminiferous aether, if I understand the situation aright, has had its day and may now be considered as mere lumber' (p. 85). 'The theory of relativity has shown conclusively that there are no gravitational "forces" in the world in the sense in which there are electric sparks' (p. 283).

[50] She denies that 'recent developments in physics have any tendency to show that materialism is false or are capable of being used to provide any arguments in favour of idealism' (p. 278). Miss Stebbing, however, exemplifies the current delusion that, while physics is impregnably barricaded with mathematical sandbags, theology is a pleasant no-man's-land. She thinks any amateur can saunter up to St. Augustine to discuss predestination (p. 231), blissfully ignorant of the enormous specialist literature on the subject.

(*a*) Rowland showed that a rotating charge deflects a magnet, as Oersted showed for an ordinary current.

> Not only does the force fail to lie on the line connecting charge and magnet, but the intensity of the force depends on the velocity of the charge. The whole mechanical point of view was based on the belief that all phenomena can be explained in terms of forces depending only on the distance and not on the velocity. . . . These experimental facts contradicted the philosophical view that all forces must act on the line connecting [the simultaneous positions of] the particles and can depend only upon [the simultaneous] distance.—pp. 93, 132.

There is a delightfully naïve flavour about this view which is asserted to be not only mechanical but philosophical! It was already denied by Gauss in 1845 and by Weber in 1848. It is, as we have shown, implicitly denied by anyone who, like Einstein himself, accepts the ordinary electron theory and *therefore* the Liénard force-formula. Moreover, the general theory of relativity itself gives a similar formula (11.28) for gravitational force! [51]

(*b*) 'Science,' we are told (p. 125), 'did not succeed in carrying out the mechanical programme convincingly; and to-day no physicist believes in the possibility of its fulfilment.' This sounds rather alarming until we find that

> a mechanical construction means, as we know, that the substance is built up of particles with forces acting along lines connecting them and depending only on the distance.—p. 123.

By a 'line connecting them' the authors mean the join of their simultaneous positions, i.e. they assert that 'mechanics' always and necessarily implies instantaneous transmission. It is so comforting to be able to foist an untenable thesis on one's opponent! But, we may ask, are elastic waves and sound outside the domain of 'mechanics'?

(*c*) Having already dealt *in extenso* with the convenient metaphor of the 'lines' or the 'field' of a vector, we can content ourselves with a brief quotation:

> The lines of force—or in other words, the field—enable us to determine the forces acting on a magnetic pole at any point in

[51] Besides, as we have shown (p. 555), Rowland's experiment gives only the time-average of the statistical force. And the force is not between the 'charge and magnet,' but between the moving charge and the moving charges constituting the magnet.

space. . . . We sandwich the concept of the field between that of the current and that of the magnetic pole in order to represent the acting forces in a simple way.—p. 135.

Excellent pedagogy, especially when one is writing for people who have no notion whatever of (i) elementary vector analysis, and (ii) the fact that there is an electronic theory of magnetism. But the following two propositions are much more doubtful; and we have already argued against them.

> A field may be regarded as something always associated [rather a vague term !] with a current. It is there even in the absence of a magnetic pole to test its existence.—p. 139.
>
> As long as a charge is at rest [in the laboratory ?] there is only an electrostatic field. But a magnetic field appears as soon as the charge begins to move.—p. 141 f.

(*d*) The problem of the *schesis* is shelved in the usual manner by rejecting something which nobody holds and by quibbling about 'space':

> Our only way out seems to be to take for granted the fact that space has the physical property of transmitting electromagnetic waves, and not to bother too much about the meaning of this statement. We may still use the word ether, but only to express some physical property of space. This word ether has changed its meaning many times in the development of science. At the moment it no longer stands for a medium built up of particles.—p. 159.

(*e*) On p. 165 we find a statement of *corelativity*:

> Physical experiments performed in a uniformly moving train or ship will give exactly the same results as on the earth. . . . This result can be expressed by the so-called Galilean relativity principle: if the laws of mechanics are valid in one C.S. [= co-ordinate system], then they are valid in any other C.S. moving uniformly relative to the first.

This principle—enunciated by Newton rather than Galileo—presupposes that the systems are *complete*. It does *not* apply, for instance, if the medium in the case of elastic propagation—or the schesis in the case of electromagnetic transmission—is not convected.

(*f*) But on the next page we begin to slide almost imperceptibly into *interrelativity*, for we are told that 'some difficulty arises if the two observers begin to discuss observations of the same event from the point of view of their different C.S.' Note also

that each system carries its own 'observer.' And the reference to 'event' raises questions about date and position. The transition is not quite as innocent as it appears!

The conclusion is given on p. 171:

> Although the co-ordinates and velocity change when passing from one C.S. to another, the force and change of velocity, and therefore the laws of mechanics, are invariant with respect to the transformation laws.

That is, the acceleration of a point is unchanged by the transformation (9.60). But a force such as ku^2 becomes $k(u-v)^2$. And such 'laws of mechanics' as the equation of a sound-wave $\Sigma(x-X)^2 = c(t-T)^2$ or equation (1.32b) *are* decidedly changed. Hence we reject the conclusion of Einstein-Infeld.

(*g*) The authors next invent an imaginary interrelative experiment (p. 172 f):

> We are sitting in a closed room so isolated from the external world that no air can enter or escape. . . . Let us now imagine that our room moves uniformly through space. A man outside sees, through the glass walls of the moving room (or train if you prefer), everything which is going on inside.

That is, the sound-medium M is convected with the system K', so that the wave-equation is (9.62). For the case of sound the authors admit the equation (9.62a) referred to K. But *not* for electromagnetics or light; when $c = 3.10^{10}$ cm./sec., the equation (9.63) holds, i.e. Voigt's transformation then applies to interrelative systems—provided, presumably, there are *two* observers. 'There is not the slightest doubt,' we are assured (p. 177), 'as to the clarity of this verdict, although it is obtained through rather indirect experiments in view of the great technical difficulties.' Well, the dogmatic certitude of this assertion leaves nothing to be desired! The present writer must humbly confess that he has for many years been searching for such experiments, direct or indirect, and has been unable to find them. So naturally we turn hopefully to the next sentences:

> We shall not go into detailed description of the many experiments from which this important conclusion can be drawn. We can, however, use some very simple arguments which, though they do not prove that the velocity of light is independent of the motion of the source, nevertheless make this fact convincing and understandable.—p. 177 f.

The authors then tell us (i) that in the case of double stars the speed of light cannot 'depend on the velocity of the emitting body,' (ii) that the ether cannot be carried round by 'a wheel rotating very quickly.'

Now was there ever such a flagrant *ignoratio elenchi*? We are told a pretty story of a relativist gaoler outside a moving glass house imprisoning a physicist; and we are vehemently assured that their observations are connected by a bit of algebra (given by Voigt in 1887) which involves a manipulation of dates and positions hitherto unknown in science. Then in justification we are given two 'very simple arguments' which suggest but 'do not prove'—not the point at issue at all, but the altogether different fact (if it be a fact) that electromagnetic transmission is medium-like! We hope elsewhere to query the validity of this argument against Ritz. At the moment we merely point out that, by accepting Liénard against Ritz, one does not thereby prove Einstein.

(*h*) We now come to the *MM′* experiment:

> In the famous Michelson-Morley experiment the result was a verdict of 'death' to the theory of a calm ether-sea through which all matter moves. . . . The situation grows more and more serious. Two assumptions have been tried. The first, that moving bodies carry ether along; the fact that the velocity of light does not depend on the motion of the source contradicts this assumption. The second, that there exists one distinguished C.S. and that moving bodies do not carry the ether but travel through an ever calm ether-sea.—p. 183.

We are in cordial agreement with the first sentence: the *MM′* experiment disproves the hypothesis of a stationary aether. Lorentz being eliminated, we have a choice between Stokes and Ritz: either the aether is earth-convected or there is no aether at all. And we have already shown that, as far as all laboratory experiments are concerned, Einstein opts for Stokes. But in the foregoing passage we find Messrs. Einstein and Infeld wringing their hands in despair: 'All assumptions concerning ether led nowhere' (p. 184). Only by examining the equations—none of which is given in the book before us—do we discover that Einstein valiantly upholds the earth-convected *schesis* plus Liénard's force-formula with its v and v' referred thereto! This simple fact is concealed from the reader—and apparently from the authors themselves—by bowdlerising Stokes's theory into

the assertion 'that moving bodies carry ether along.' That is, a glasshouse moving across the earth, or a piece of dielectric moving through the laboratory, convects the schesis. No one—not even Hertz—maintained this hypothesis; and it has no connection whatever either with double stars or with the MM' or any electromagnetic experiment. We therefore reject the false dilemma: *Aut Einstein aut nullus*!

In view of our previous lengthy discussion it is unnecessary to pursue our criticisms further. We therefore forgo any comments on what is said concerning moving rods and clocks, the mass-velocity law, the alleged identification of mass and energy, space-time, and so on. Our arguments are now before our readers; and they must decide whether they are valid as against what—if we may judge from the Einstein-Infeld book—is regarded as contemporary orthodoxy in physics.[52]

[52] A brief reference may be made to some subsequently issued articles: (1) On p. 330 we stated that the formula $p = p_0/\beta$ had not been verified experimentally; independently of this, we rejected Einstein's date argument. H. E. Ives—*Nature* 141 (1938) 551—claims to have observed this very minute effect in canal rays. And, very curiously, he regards this alleged 'transverse Doppler effect' as 'decisive in favour of the Larmor-Lorentz theory' (whatever that is) against 'an entrained ether.' (2) 'A critical analysis of the classical experiments on the relativistic variation of electron mass' has been published by C. T. Zahn and A. H. Spees in PR 53 (1938) 511–521. They hold that there has as yet been no satisfactory discrimination 'between the Abraham and the Lorentz electrons.' (3) Milne's *a priori* reconstruction of physics has now brought him to 'the equations of electromagnetism'—PRS 165A (1938) 313–357. We propose to investigate his arguments elsewhere; but our arguments against moving observers and dates are relevant. We cannot admit his new law of force, nor do we think he has solved the difficulty which we mentioned on p. 498 above.

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This was originally compiled to include both works on electromagnetics and the most important or most representative publications on the special theory of relativity. As the material proved to be too bulky for one volume, the references to relativity have been removed. The bibliography is, of course, not intended in any sense to be exhaustive. It is designed to include (*a*) publications of fundamental importance in the development of electromagnetic theory, (*b*) references to some experimental results of theoretical significance, (*c*) representative text-books which illustrate the prevalent views, (*d*) some of the more recent discussions on units and dimensions. The earlier literature is catalogued in P. F. Mottelay, *Bibliographical History of Electricity and Magnetism*, London, 1922. The following abbreviations are employed:

AP	Annalen der Physik.
ANSE	Archives néerlandaises des sciences exactes et naturelles.
BNRC	Bulletin of the National Research Council (Washington).
CR	Comptes rendus de l'Académie des Sciences (Paris).
DAP	Dictionary of Applied Physics (ed. Glazebrook), vol. 2 (Electricity) 1922.
ETZ	Elektrotechnische Zeitschrift.
Geiger-Scheel	Handbuch der Physik.
JfM	Journal für reine und angewandte Mathematik.
JP	Journal de Physique.
MA	Mathematische Annalen.
PM	Philosophical Magazine.
PPS	Proceedings of the Physical Society (London).
PR	Physical Review.
PRS	Proceedings of the Royal Society.
PT	Philosophical Transactions.
PZ	Physikalische Zeitschrift.
Taylor	Scientific Memoirs selected from the Transactions of Foreign Academies of Science and Learned Societies and from Foreign Journals. Ed. R. Taylor. 5 vols. London, 1837–52.
Wien-Harms	Handbuch der Experimental-Physik.
ZfP	Zeitschrift für Physik.

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